Journal
of
The Franklin Institute

EDITOR, HOWARD McCLENAHAN, E.E., LITT.D., D.ENG., D.SC., LL.D.
ASSISTANT EDITOR, ALFRED RIGLING, M.A.

Associate Editors:

JOSEPH S. AMES, PH.D.
WILDER D. BANCROFT, PH.D.
E. J. BERG, SC.D.
E. G. COKER, F.R.S.
ARTHUR L. DAY, SC.D.
A. S. EVE, F.R.S.
PAUL D. FOOTE, PH.D.
W. J. HUMPHREYS, PH.D.
A. E. KENNELLY, SC.D.
C. E. K. MEES, D.SC.
RALPH MODJESKI, D.ENG.
W. F. G. SWANN, D.SC.
D. W. TAYLOR, LL.D.
A. F. ZAHM, PH.D.
JOHN ZELENY, PH.D.

Committee on Publications:

G. H. CLAMER, CHAIRMAN
A. W. GOODSPEED
CLARENCE A. HALL
GEORGE A. HOADLEY
MALCOLM LLOYD, JR.

Vol. 218 **SEPTEMBER, 1934** **No. 3**

CONTENTS

Theory of Servo-Mechanisms ..... H. L. HAZEN 279

The All Steel Automobile Body from Carriage Lines to Streamlines. GRISCOM BETTLE 333

The Physical Cause Back of the Relativity Equations ..... JAMES MACKAYE 343

Notes from the U. S. Bureau of Standards 381

The Franklin Institute Notes 385

Book Reviews 386

Publications Received 392

Current Topics 332, 342, 384, 394

Entered at the Post Office at Lancaster, Pa., March 14, 1928, as second-class matter under act of March 3, 1879. Acceptance for mailing at special rate of postage provided for in section 1103, Act of October 3, 1917, authorized on July 3, 1919.

Published by

THE FRANKLIN INSTITUTE OF THE STATE OF PENNSYLVANIA

Prince and Lemon Streets, Lancaster, Penna., and

Twentieth Street and Parkway, Philadelphia, Penna.

SIX DOLLARS PER YEAR (Foreign Postage Additional) **SINGLE NUMBERS, SIXTY CENTS**

Indexes to the semi-annual volumes of the JOURNAL are published with the June and December numbers. The contents are also indexed in *The Industrial Arts Index.*

A Complete Plant Under One Roof

PRINTING - BINDING - ELECTROTYPING

Printers of
Scientific and Technical Journals and Books
Theses and Dissertations
Works in Foreign Languages

Your Book, Journal or Thesis placed with us insures that the composition, proof-reading, electrotyping, presswork and binding, follow through in consecutive order in one plant—established fifty-seven years ago—and under the supervision of one management.

LANCASTER PRESS, Inc.

Established 1877 **LANCASTER, PA.**

[*Printers of Journal of The Franklin Institute*]

JOURNAL OF THE FRANKLIN INSTITUTE

For March, 1926, April, 1929, March, 1930, and Sept., 1930

THE INSTITUTE'S SUPPLY OF COPIES of the Journals for March, 1926, April, 1929, March, 1930, and Sept., 1930, is exhausted. The Editors will be very grateful to any Members who do not wish these numbers for their own files if they will send them to the Institute. Compliance with this request will be very deeply appreciated.

Rebuilding ~ Remodeling ~ Reconstructing

HARRY R. RUST

INCORPORATED

Contractors, Carpenters, Builders

INTERIOR WOODWORK : STORE FIXTURES : BULK WINDOWS

Office: 19 S. 8th ST. ***Mill:*** 724-6-8 LUDLOW ST.
Shop: 41-43-51 HUTCHINSON ST.
PHILADELPHIA, PA.

Journal
of
The Franklin Institute
Devoted to Science and the Mechanic Arts

Vol. 218 SEPTEMBER, 1934 No. 3

THEORY OF SERVO-MECHANISMS.

BY

H. L. HAZEN, Sc.D.,

Massachusetts Institute of Technology.

INTRODUCTION.

In this age characterized by huge resources of mechanical and electrical power, these agencies have in many fields almost completely replaced human muscular power. In a similar way the functions of human operators are being taken over by mechanisms that automatically control the performance of machines and processes. Automatic control is often more reliable and accurate, as well as cheaper, than human control. Consequently the study of the performance of automatic control devices is of particular interest at the present time.

Automatic control devices are of two principal kinds. In the first kind the control is actuated by some quantity such as time which is more or less independent of the result of the control operation. Time-operated traffic-signal control is an example of this first kind. With this type of control the flow of traffic resulting from the action of the signal lights in no way affects the cycle of operation of the lights. In the second kind, the control is actuated by a quantity that is affected by the control operation, and for this reason it may be called a "closed-cycle" control. An example of this kind is the thermostat in which the temperature-sensitive element controls the amount of heat supplied to some object such as a process furnace or a house, while the temperature of the object actuates the thermostat. The first kind of control may be superimposed upon the second kind as in the case of a house

(Note—The Franklin Institute is not responsible for the statements and opinions advanced by contributors to the JOURNAL.)

thermostat whose setting is changed periodically by a clock. The second kind is also illustrated by certain traffic light controls in which the cycle of light operation is automatically adjusted by the flow of traffic. Another example of the second kind of control is automatic ship steering. Here the rudder operation is actuated by the angle between compass and hull, while this angle is in turn affected by the rudder operation.

The distinction between these two kinds of control is made because of important differences in the nature of the control mechanism required in the two cases. In the first kind the source of the control usually has sufficient power to operate the control mechanism directly. Thus synchronous motors are available which can cheaply and accurately effect time control for practically any device. In the second kind, however, the source of control is some form of measuring instrument which very seldom can deliver any appreciable force or power and give usefully accurate indications. The operation of the actual control element nearly always requires much greater power than that available from the measuring instrument. Consequently some intermediate device for amplifying the power of the measuring instrument, while preserving its indications, is essential. When the output element of such a device is so actuated as to make the difference between the output and input indications tend to zero the device is called a "follow up" mechanism or servo-mechanism. In what follows a "servo-mechanism" is frequently called merely a "servo."

In the thermostat or controlling pyrometer,[1, 2, 3] the measuring instrument takes various forms. One of the simplest is the bimetal element or other differential expansion device in which the indication is a mechanical displacement. Other forms are the thermocouple and the temperature-sensitive resistance in which the indication appears as an electric potential difference or current. In nearly all cases, the energy associated with these indications is very small while the actual control is effected by a valve, switch, rheostat, damper, or other device requiring considerable energy for its operation. A servo-mechanism or other amplifier bridges this gap in energy level.

[1] For numbered references see bibliography.

Considering the ship-steering example, this disparity exists between the energy magnitude associated with the measuring instrument, a compass, and that associated with driving the rudder. In small craft with a direct wheel-to-rudder drive, the helmsman serves as a human servo-mechanism. In larger craft, the steering engine is a servo-mechanism between wheel and rudder, while the helmsman is still a human servo-mechanism between compass and wheel. With automatic steering the helmsman is replaced by a servo-mechanism which in turn controls the steering engine. Alternatively, the entire compass-to-rudder drive may be considered as a single servo-mechanism.[4, 5, 6, 7, 8]

Thus it is seen that a servo-mechanism is very likely to form an essential part of any closed-cycle control system. Servo-mechanisms are also used merely to produce the indications of measuring instruments at a relatively high power level. Many recording instruments contain illustrations of this use.[1, 2, 3, 22, 23]

Applications of servo-mechanisms, in addition to those already mentioned, include the speed control of steam turbines and water wheels by governors;[9] the control of these governors by master clocks or by power-indicating instruments;[9, 10, 11, 12] the stabilization of ships by gyroscopes;[13, 14, 15, 16, 17] the operation of gyro-compass repeaters;[6, 7] the automatic stabilization and guiding of aircraft;[18, 19, 20, 21] and in fact the automatic recording or control of almost any measurable or measurable and controlable physical quantity.[1, 2, 3, 22, 23, 24, 25, 26, 27, 28]

It is apparent that servo-mechanisms form a vital link in the application of automatic control and that a study of their performance is of importance. The purpose of this paper is to present an analysis of the operating characteristics of certain important types of servo-mechanisms.

Although the subject of servo-mechanisms has been treated in a qualitative or semi-quantitative way in some of the references given above, to the writer's knowledge no systematic quantitative treatment of even the simple common types has previously been given.* It seems worth while, therefore, in

* Minorsky's first paper, ref. 4, gives an excellent analysis of the rudder-hull dynamic system in the ship-steering problem which bears some resemblance to the problem of the continuous-control type of servo here treated. Specific references to his results are given later in the paper.

view of the rapidly expanding field of application of servo-mechanisms, to present the beginnings of such a treatment, outlining quantitatively at least a few of the important properties of the familiar types. Preceding the quantitative treatment, a general discussion of servos and of properties of three important types will be given.*

GENERAL CHARACTERISTICS OF SERVO-MECHANISMS.

Before going into further detail regarding the performance of servos it will be well to define more explicitly what is meant by the term servo-mechanism. As stated before, a servo-mechanism is a power-amplifying device. However, its action differs in one essential particular from that of a simple vacuum-tube or mechanical power amplifier.† Such an amplifier preserves approximately a given functional relation, usually linear, between input and output quantities, due to the properties of the amplifier itself. Thus in a good vacuum-tube amplifier, the current output is very closely proportional to the voltage applied to the grid of the first tube. This linearity of response is due to the constancy of the parameters within the amplifier. Any departure from constancy of these parameters affects the relation between input and output directly.

The servo-mechanism differs from the simple amplifier in that the responsibility for the functional relation is not placed directly on the amplifying element of the servo. Here it will be necessary to distinguish between the input to the servo-mechanism, which is the indication of the measuring instrument, and the input to the amplifier element in the servo which is something different. The output of the servo amplifier element can be considered as the output of the servo, however. In a servo-mechanism, the input to the servo amplifier element is connected to the *difference* between the servo input and output. When this difference is finite the servo output is driven in such a manner as to tend to make this difference zero. Thus the only function of the servo am-

* In a companion paper, the design and test of a high performance servo-mechanism, designed on the basis of the analysis in the present paper, is given.

† For a description of an ingenious and successful mechanical torque (and hence power) amplifier developed by Mr. Neiman of the Bethlehem Steel Corporation see reference 29, bibliography.

plifier element is to apply sufficient force to the servo output to bring it rapidly to correspondence with the servo input. Such an amplifier element can be a relatively crude affair. In fact it may consist merely of a suitable relay or switch controlling an electric motor. An illustration of this action is furnished by a servo-mechanism used on an early model of a machine for solving differential equations.[30] The servo input was the angle of a rotating shaft, the output was the angle of another rotating shaft. When these two angles differed, a contact started the electric servo-motor which drove the output shaft in the direction to restore coincidence of the angles.

A servo-mechanism *may thus be defined as a power-amplifying device in which the amplifier element driving the output is actuated by the difference between the input to the servo and its output.* An ideal servo-mechanism is one in which the input and output indications (expressed in common units) are equal at all instants of time. Although the ideal servo is never realized in practice, its operation furnishes the standard by which the operation of actual servos is judged.

When compared to the ideal, an actual servo-mechanism is subject to two principal defects, oscillation and lag. Oscillation is a periodic deviation of output from input. Lag is an average or unidirectional deviation. Either or both of these forms of deviation are always present in some degree and it is the purpose of design to reduce these deviations to a magnitude which is negligible in any particular application.

A servo-mechanism is by nature the type of device in which oscillation would be expected to occur if definite preventative means were not employed. To simplify the discussion, assume that the input indication is a constant. The output is acted upon by a force tending to return it to coincidence with the input. In the absence of damping forces such a system would oscillate indefinitely at any amplitude at which it was started. Suitable damping reduces these oscillations to a small amplitude or prevents them altogether, depending on the type of servo. In certain applications, small oscillations may be useful in minimizing the effects of friction by making the dynamic rather than the larger static coefficient effective. At best this scheme reduces one evil at the cost of

introducing another. The friction evil might better be treated by increasing the ratio of driving torque to friction torque. Continuous oscillation necessarily introduces relatively large wear and tear in moving parts. Oscillation, then, is to be tolerated only as an evil justified by cost or by being the lesser of two optional evils.

Lag occurs in some degree in practically all servo-mechanisms when the input has motion. That is, the output indication at a given instant corresponds to that of the input a short time before. In some applications the effect of lag may be negligible while in others it may be very serious.[30] It does no harm, for example, in a recording-instrument servo provided the magnitude of the lag is less than the permissible error, and provided this source of error is taken into account in the design of the instrument. In a closed-cycle control system, however, lag in the servo-mechanism frequently introduces negative damping into the system as a whole, which must be overbalanced by positive damping from some other source if the system is to be stable.* Consequently it is an effect that must be carefully considered.

The quantitative study of oscillation and lag requires a separate analysis for each type of servo. Some servos are inherently oscillatory while others can be made non-oscillatory by suitable design. Three main types in common use are treated in this paper and will be described briefly before proceeding to the mathematical analysis.

The first type of servo-mechanism and one that is widely used because of its simplicity, may be called a relay servo, because of the essentially "off" and "on" nature of the forces acting on the output element. Several forms of gyro-compass repeaters, automatic pilots, and gyro-stabilizers for ships use this type of servo.[6, 13, 14, 15, 18, 20, 21, 27] In this type the restoring force applied to the output element is usually substantially constant in magnitude, while operative. Ideally, this force would be brought into operation by an infinitesimal deviation of the output from the input but practically it is usually necessary to have a small range of deviation over which the restoring force is inactive. If the inactive deviation range were infinitesimal, and if there were no time lag in the applica-

* For example see ref. 4 (1922), pp. 305–8, and ref. 30.

tion of the restoring force, this servo could be made to operate with an infinitesimal amplitude of oscillation, an infinite frequency of oscillation, and an infinitesimal lag error. It is quite evident that such operation represents a limiting case which at best can only be roughly approached with actual physical apparatus. Practically the amplitude and frequency of oscillation are finite, and in most cases lag error is present. The limiting case is of interest however from the point of view of analysis and from its significance as an ideal.

This type of servo has certain disadvantages. The rather sudden application of the entire available driving torque, first in one direction and then in the other is conducive to large wear and tear of the entire mechanism. In practice the restoring force is usually initiated by electric contacts which may be somewhat troublesome especially when only very small forces are available for their operation. This type of servo has the asset of simplicity, however, and may be useful where static friction in the mechanism is troublesome, and a relatively crude type of control suffices.

The second type of servo-mechanism is one in which the correction of the output is made in finite steps at definite time intervals. This type is extensively used in recording instruments and controllers for quantities that vary relatively slowly.[1, 2, 3, 9, 10] It is well illustrated by a temperature recorder in which the position of the pen is periodically tested to ascertain whether or not it corresponds to the indicated temperature. If in error, the pen position is given a small finite change in the correct direction and a short time later its position is again tested, and so on. In this scheme, the magnitude of the step correction is usually made approximately proportional to the deviation between output and input. When properly designed and adjusted this type of servo is non-oscillatory. The necessary conditions for non-oscillation are: first, that any correction and the indication of it shall be substantially completed before a new test is made; second, that the correction applied shall not be greater than that required to reduce the deviation approximately to zero; and third, that the inactive range over which the output can vary without causing a correction to be made shall be at least as large as the smallest possible correction step.

A modification of this type is used in an automatic steering gear suitable for small craft.[8] The rudder, which is the output of the servo, is periodically displaced from mid position an amount approximately proportional to the error in the ship's heading, and then allowed to trail back to mid position before the heading is again tested and another temporary displacement is given to the rudder.

Evidently the finite-step or impulse type of servo-mechanism can be non-oscillatory. However it does have a persistent lag error when the input is so varying that corrections must be applied continually in one direction. It is also inherently somewhat slower than the other types of servos, since in order to be non-oscillatory, the driving force can be effective during only a fraction of the cycle of operation.

The third type of servo considered here is one in which the restoring force, acting continuously on the output element, is approximately proportional to the deviation of the output.

This type is rapidly coming into use and is now employed in a number of devices such as certain gyro-compass followers,[6, 7] and various automatic recorders.[22, 23, 24, 25, 26] By the use of suitable damping, its action can be rendered aperiodic or oscillatory to any desired degree. Where a high speed of response, high sensitivity, and freedom from hunting are desired this type undoubtedly has the greatest possibilities of the three. Small deviations call into play only small restoring forces, hence its operation lacks the somewhat violent nature characteristic of relay servos. Moreover, these small forces can be called into play promptly and large deviations are thereby avoided. This type can be built to have very rapid response.

A servo-mechanism of this third type, described in a companion paper, capable of operating on the output of a photo-electric cell and of delivering about one-tenth of a horse power has been built which substantially completes the correction of a small deviation in about one-twentieth of a second. If desired this speed could undoubtedly be improved by a factor of five using the same basic design. This servo-mechanism as normally operated is aperiodic in response.*

---

*See footnote on p. 282.

Having discussed the general characteristics of servo-mechanisms, and the method of operation of three of the most important types, attention is turned to the analysis. In what follows only a very simple dynamic system is treated in detail, one in which friction, inertia, and a restoring force are associated with the output element. This representation is very close to the truth for many servos. For example, in the case of the continuous-control servo referred to above, test and calculation agreed within a small experimental error. In other cases, the idealizations made in order to make a usefully simple analytical treatment possible may depart somewhat more from the facts. Nevertheless, the analysis of an idealized case gives a real insight into the characteristics of a given servo. In most specific cases in which a general analytical solution is too cumbersome to be useful, a restricted analytical or a numerical solution can be readily made, taking into account departures from more idealized conditions. The methods of treating these more involved cases will be outlined briefly at suitable points in the analysis. The first type to be considered is the relay-servo.

RELAY-TYPE SERVO-MECHANISMS.

The relay type of servo-mechanism is characterized primarily by a constant-magnitude restoring force brought into play when the output deviates by some predetermined amount from the input. This characteristic alone does not fix the performance however as the effects of a number of other factors are important. Among these others are included: first, the type of dynamic system to which this force is applied, which includes one or more inertia and friction parameters, and possibly elastance parameters; second, the nature of the friction parameters, whether the frictional force is independent of, or a function of, the output velocity; third, the inactive range, or the limits of the output deviation within which the restoring force is not in action; fourth, the time delay between the indication that the restoring force is to be applied or removed, and the actual application or removal of this force; fifth, the nature of variation of the input, whether this variation is relatively slow so that the servo maintains the output at a substantially constant value, or is rapid and unidirectional

so that the servo must move the output more or less continuously in one direction. Each of these factors must be considered in the analysis of any particular case. A comprehensive treatment of all cases is evidently beyond the scope of this paper, but a few significant cases will be analyzed and from the results obtained deductions can be made covering other cases.

In this section an analysis is made of the cases indicated in the following outline:

Relay Servo-Mechanisms.

A. No inactive zone.
  1. Friction force proportional to velocity (viscous friction).
  2. Friction force independent of velocity (Coulomb friction).

B. Finite inactive zone.
  1. Friction force proportional to velocity.
  2. Friction force independent of velocity.

C. Cases *A* and *B* with time delay.

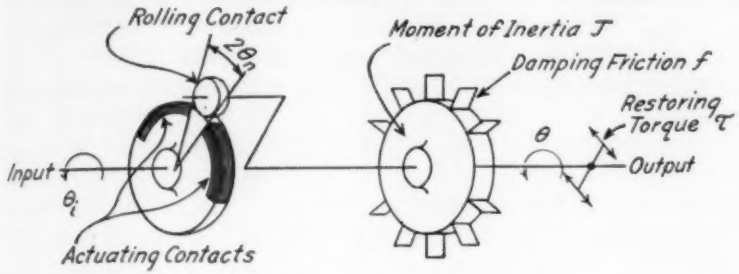
The properties of particular interest are the amplitude of steady-state oscillation, and the magnitude of the average lag error as a function of the input speed. These quantities will be determined in what follows.

Because the output of many servos is in the form of mechanical rotation the analysis is given in these terms. However, the equations can be used as given for certain other systems, by merely redefining the symbols in terms of the analogous parameters of other types of motion such as mechanical translation or electric current. A more specific discussion of this subject is given further on in the paper.

Case A1. Relay-type servo having no inactive zone and output-element characterized by inertia and viscous friction. Three different conditions of operation of this servo will be considered: first, stationary input and zero time lag in application of restoring force; second, stationary input, and finite time lag; third, uniformly varying input, zero time lag. For each of these conditions the amplitude of steady-state oscillation and the lag error will be determined.

For the first condition consider the action of the physical system shown schematically in Fig. 1.

FIG. 1.

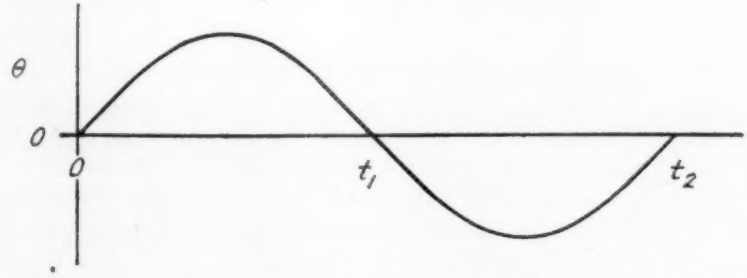


Schematic diagram of the significant dynamic elements of the relay-type servo-mechanism.

Let θ_i = angle of servo input (radians),
θ = angle of servo output (radians),
$2\theta_n$ = inactive range (radians) (= 0 for case A),
J = moment of inertia of servo referred to output (gm. cm.^{2}),
f = friction or damping torque per unit angular velocity of output (dyne cm. per radian per sec.),
$\pm \tau$ = restoring torque acting on output (dyne cm.),
$p = \frac{d}{dt}$, the time derivative operator,
$\omega = p\theta$ = angular velocity (radians per sec.).

The restoring torque τ is normally of sign opposite to that of the quantity $\theta_i - \theta$. A sketch of θ for one cycle of steady-state operation is shown in Fig. 2. To find the amplitude of

FIG. 2.



steady-state oscillation it is necessary merely to solve for the motion of the output over the range $0 < t < t_1$ in terms of

ω_0, the value of $p\theta$ at $t = 0$, and to make $p\theta$ at t_1 equal to $-\omega_0$. The resulting equation in ω_0 and the system parameters determines ω_0 for steady-state operation.

For the interval $0 < t < t_1$ the differential equation of motion is

$$-\tau - fp\theta = Jp^2\theta. \tag{1}$$

The known terminal conditions are

$$\left.\begin{aligned} t &= 0 \\ \theta &= 0 \\ \omega &= \omega_0 \end{aligned}\right\}, \tag{2}$$

$$\left.\begin{aligned} t &= t_1 \\ \theta &= 0 \\ \omega &= -\omega_0 \end{aligned}\right\} \tag{3}$$

Integrating (1) and inserting conditions (2) there results

$$\omega = -\omega_s + (\omega_s + \omega_0)\epsilon^{-t/T}, \tag{4}$$

$$\theta = -\omega_s t + T(\omega_s + \omega_0)(1 - \epsilon^{-t/T}), \tag{5}$$

in which

$$\omega_s = \frac{\tau}{f} = \text{runaway velocity of output},$$

$$T = \frac{J}{f}.$$

Solving (4) for t_1 with $\omega = -\omega_0$ and using this value of t_1 in (5) for which $\theta = 0$, there results

$$\ln \frac{1 - \dfrac{\omega_0}{\omega_s}}{1 + \dfrac{\omega_0}{\omega_s}} + 2\frac{\omega_0}{\omega_s} = 0. \tag{6}$$

Equation (6) is satisfied only for vanishingly small values of ω_0/ω_s. Since ω_s is finite, ω_0 must be vanishingly small. From this result it is seen that the servo output in this case oscillates, but at an infinitesimal amplitude and an infinite frequency. Physically this result is absurd for any actual servo, hence the necessary conclusion is that this case is too greatly idealized to represent the facts. It is of interest, however, as a limiting case.

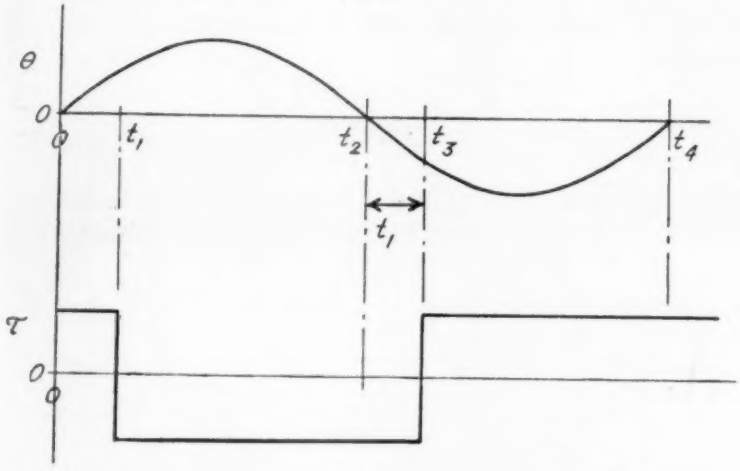
Because (6) is very nearly satisfied for small finite values of ω_0/ω_s, it is to be inferred that only a slight departure from

the assumed conditions could result in a finite amplitude and frequency of oscillation.

By taking time lag in the change of the restoring force into account, a practical case results. As the second condition then the results of time lag will be considered.

This condition differs from the one just treated only in that the restoring torque is changed in sign, not when $\theta = 0$ but t_1 seconds later. A cycle of output angle and restoring torque is shown in Fig. 3a. The plan of solution is similar to that just used.

FIG. 3.



Restoring torque and output angle for case A1 with time lag present. Input stationary.

For the interval, $0 < t < t_1$, the differential equation of motion is

$$\tau - fp\theta = Jp^2\theta. \tag{7}$$

For the interval $t_1 < t < t_3$ this changes to (1). The known terminal conditions are:

$$\left.\begin{array}{l} t = 0 \\ \omega = \omega_0 \\ \theta = 0 \end{array}\right\} \quad (8) \qquad \left.\begin{array}{l} t = t_2 \\ \omega = \omega_2 = -\omega_0 \text{ for steady state} \\ \theta = 0 \end{array}\right\} \quad (9)$$

and that at $t = t_1$, ω and θ are continuous. Integrating (7) and (1), inserting the terminal conditions (8) and (9), and

equating the two resulting expressions for ω at $t = t_1$, there results

$$\epsilon^{-t_1/T} + \epsilon^{(t_2-t_1)/T} = \frac{2\omega_s}{\omega_s - \omega_0}. \tag{10}$$

Equating the two expressions for θ at $t = t_1$,

$$\omega_s(2t_1 - t_2) + T(\omega_0 - \omega_s)(2 - \epsilon^{-t_1/T} - \epsilon^{(t_2-t_1)/T}) = 0. \tag{11}$$

Substituting (10) in (11), solving the result for t_2 and putting this in (10), gives, after some algebraic reduction, the following equation in t_1 and ω_0:

$$a^2(1 - \Omega_0)\epsilon^{2\Omega_0} - 2a + (1 - \Omega_0) = 0, \tag{12}$$

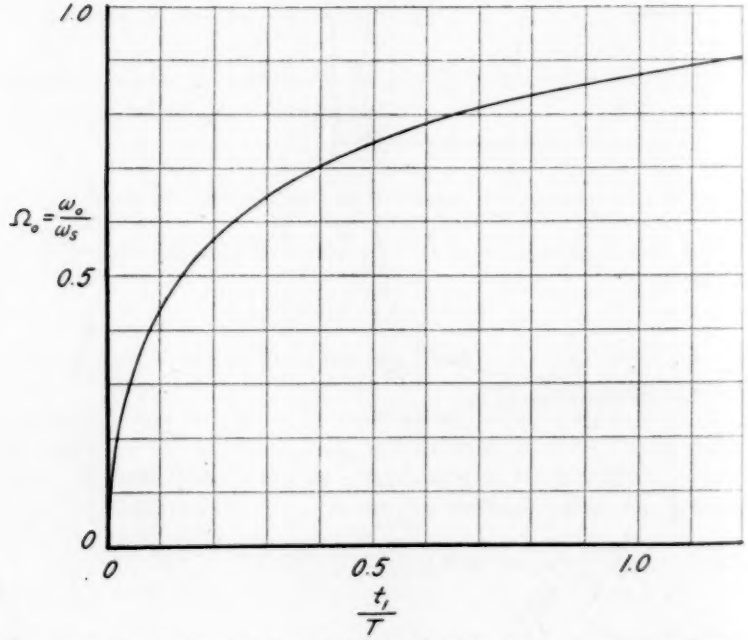
in which

$$a = \epsilon^{t_1/T},$$

$$\Omega_0 = \frac{\omega_0}{\omega_s}.$$

This relation between ω_0 and t_1 plotted in dimensionless form as Ω_0 vs. t_1/T is shown in Fig. 4. The conclusion drawn by

FIG. 4.



Amplitude of steady-state oscillation as a function of time lag for case A1. Input stationary.

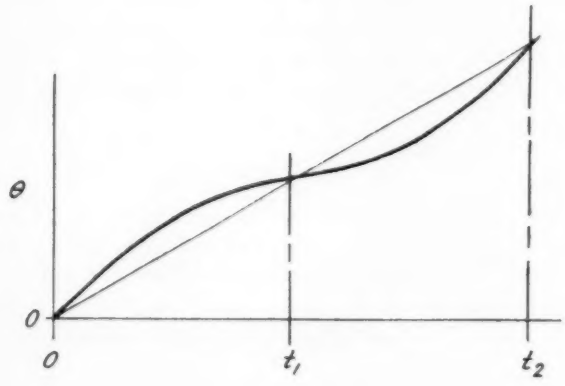
inference from (6) above that ω_0 might acquire a relatively large finite amplitude for even a small departure from the assumed ideal conditions is well borne out by Fig. 4. Evidently if a small amplitude is desired from this type of servo, it is very important to make the time lag in the application of the restoring-torque change very small. This lag is the effective lag due to all causes such as the time required to actuate relays and that required to establish a torque after the necessary contacts, or valves, etc., have been operated.

The third condition will next be considered. This condition is similar to the first, i.e., time lag is not considered, but the input instead of being stationary is moving at a constant speed,

$$\left.\begin{aligned} p\theta_i &= \omega_a \\ \theta_i &= \omega_a t \end{aligned}\right\}, \tag{13}$$

where ω_a is constant. A curve of the input angle θ_i and the output angle θ for a sample cycle beginning at $t = 0$ is shown in Fig. 5. The differential equations of motion are

FIG. 5.



$$-\tau - fp\theta = Jp^2\theta \quad \text{for} \quad 0 < t < t_1 \tag{14}$$

and

$$\tau - fp\theta = Jp^2\theta \quad \text{for} \quad t_1 < t < t_2. \tag{15}$$

For steady-state operation the known terminal conditions are

$$\left.\begin{aligned} t &= 0 \\ \omega &= \omega_0 \\ \theta &= 0 \end{aligned}\right\}, \quad (16) \qquad \left.\begin{aligned} t &= t_1 \\ \omega &= \omega_1 \\ \theta &= \omega_a t_1 \end{aligned}\right\}, \quad (17) \qquad \left.\begin{aligned} t &= t_2 \\ \omega &= \omega_0 \\ \theta &= \omega_a t_2 \end{aligned}\right\}. \quad (18)$$

Intergrating (14) and using the terminal conditions (16), there results for $0 < t < t_1$:

$$\omega = -\omega_s + (\omega_s + \omega_0)\epsilon^{-t/T}, \quad (19)$$

$$\theta = -\omega_s t + T(\omega_s + \omega_0)(1 - \epsilon^{-t/T}). \quad (20)$$

For the interval $t_1 < t < t_2$ the corresponding expressions as obtained from (15) and (17) are

$$\omega = \omega_s - (\omega_s - \omega_1)\epsilon^{-(t-t_1)/T}, \quad (21)$$

$$\theta = \omega_s(t - t_1) + \omega_a t_1 - T(\omega_s - \omega_1)(1 - \epsilon^{-(t-t_1)/T}). \quad (22)$$

From the relations (17) to (22) the steady-state amplitude of oscillation can be obtained in terms of the relation of ω_0 and ω_1 to ω_a, that is in terms of the relation of the maximum and minimum angular velocities to the average angular velocity of the output. Let

$$\left.\begin{aligned} \frac{\omega_a}{\omega_s} &= \Omega_a \\ \frac{\omega_0}{\omega_s} &= \Omega_0 \\ \frac{\omega_1}{\omega_s} &= \Omega_1 \end{aligned}\right\}. \quad (23)$$

Using (17) in (19) and expressing the result in terms of (23),

$$\epsilon^{-t_1/T} = \frac{1 + \Omega_1}{1 + \Omega_0}. \quad (24)$$

(17) in (20) gives

$$\frac{t_1}{T} = \frac{1 + \Omega_0}{1 + \Omega_a}(1 - \epsilon^{-t_1/T}). \quad (25)$$

Using (24) in (25) and solving the result for t_1/T, and sub-

stituting this in (24) gives one equation relating Ω_a, Ω_0 and Ω_1 thus:

$$\frac{1 + \Omega_1}{1 + \Omega_0} = \epsilon^{-(\Omega_0 - \Omega_1)/(1+\Omega_a)}. \tag{26}$$

Another expression relating these same quantities can be obtained from (21) and (22) with (18). Using (18) in (21),

$$\epsilon^{-(t_2 - t_1)/T} = \frac{1 - \Omega_0}{1 - \Omega_1}. \tag{27}$$

(18) in (22) gives

$$\frac{t_2 - t_1}{T} = \frac{1 - \Omega_1}{1 - \Omega_a}(1 - \epsilon^{-(t_2 - t_1)/T}). \tag{28}$$

Using (27) in (28) and solving the result for $(t_2 - t_1)/T$ and substituting this in (27) gives the second equation relating Ω_0 and Ω_1 to Ω_a. This equation is

$$\frac{1 - \Omega_0}{1 - \Omega_1} = \epsilon^{-(\Omega_0 - \Omega_1)/(1-\Omega_a)}. \tag{29}$$

Equations (26) and (29) taken simultaneously suffice to determine Ω_0 and Ω_1 in terms of Ω_a. No general analytical solution can be obtained, at least by elementary means. However from the result obtained for the previous case, i.e., for $\Omega_a = 0$ it is worth trying to see if a small amplitude of oscillation satisfies (26) and (29). To do this let

$$\begin{aligned} \Omega_0 &= (1 + \rho_0)\Omega_a, \\ \Omega_1 &= (1 - \rho_1)\Omega_a, \end{aligned} \tag{30}$$

where ρ_0 and ρ_1 are small positive quantities. Furthermore if ρ_0 and ρ_1 are small, they are presumably approximately equal. If a solution to (26) and (29) can be obtained on the assumption that they are equal, this assumption will be justified. Assume tentatively then that

$$\rho_0 = \rho_1 = \rho. \tag{31}$$

Using (30) and (31) in (26) and (29), the following expressions are obtained:

$$\frac{1 - m\rho}{1 + m\rho} = \epsilon^{-2m\rho}, \tag{32}$$

$$\frac{1 - n\rho}{1 + n\rho} = \epsilon^{-2n\rho}, \tag{33}$$

in which

$$m = \frac{\Omega_a}{1 + \Omega_a}, \qquad n = \frac{\Omega_a}{1 - \Omega_a}.$$

Expanding the left side of (32) by division and the right side by the exponential series,

$$1 - 2m\rho + 2m^2\rho^2 - 2m^3\rho^3 + \cdots = 1 - 2m\rho + \frac{4m^2\rho^2}{2} - \frac{8m^3\rho^3}{6} + \cdots. \tag{34}$$

Doing the same with (33),

$$1 - 2n\rho + 2n^2\rho^2 - 2n^3\rho^3 + \cdots = 1 - 2n\rho + \frac{4n^2\rho^2}{2} - \frac{8n^3\rho^3}{6} + \cdots. \tag{35}$$

(34) and (35) are satisfied for vanishingly small values of $m\rho$ and $n\rho$ and are approximately satisfied for values of these quantities for which their cubes are negligible in comparison with unity. Barring the limiting case in which $\Omega_a = 1$, i.e., $\omega_a = \omega_s$, (34) and (35) are satisfied by vanishingly small values of ρ. Since ρ is the maximum fractional deviation of the instantaneous output angular velocity from the average or input velocity, it is seen that the oscillations are of vanishingly small amplitude and of infinite frequency.

Although as in the first condition, this is an idealized case that cannot be realized physically, the result has interest and significance. It shows that an ideal relay-type servo is capable of following a constant input velocity with precision, i.e., without finite deviation in the nature of lag or oscillation. That this condition could be approached even under ideal conditions is interesting.

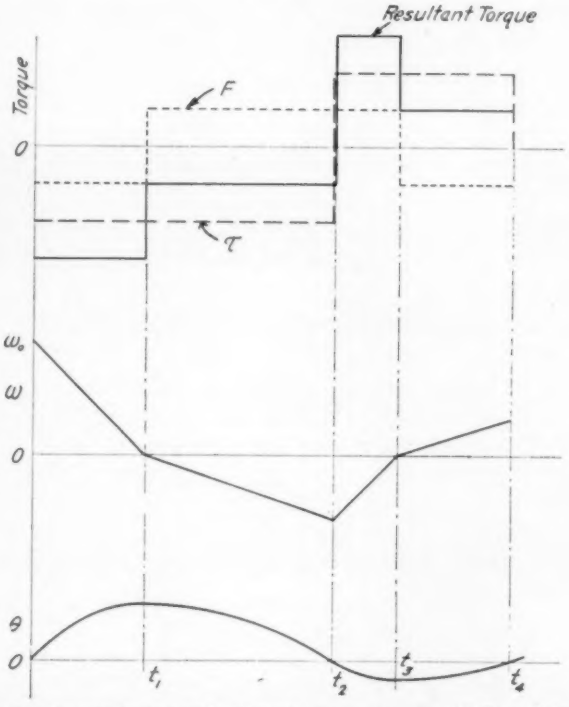
The inevitable presence of time lag in the change of the restoring torque causes a finite amplitude and frequency in any actual servo. This fact could be demonstrated by explicit formulation but the analysis will not be given here. Instead the effect of such time lag in this and other cases in which the analysis is unwieldy is discussed further on in the paper.

Case A2. In the case just treated, the friction or damping force was assumed to be proportional to the velocity. In the

present case, the frictional force is assumed to follow the Coulomb law, that is, to be independent of velocity. Otherwise the two cases are identical. As was done above, the response of the servo output for the condition of a stationary input will be treated first. As a second condition the effect of time lag is considered with the input stationary. A third condition with constant input velocity and zero time lag is considered last.

In Fig. 6 the variations of the torques, the output angle

FIG. 6.



Torques and angular velocity and angle of output for case A2. No time lag. Input stationary.

and the output velocity during a cycle of operation for the first condition are sketched. As before, the amplitude of steady-state oscillation and the output lag are the quantities of interest.

Let

$\pm F$ = friction force, sign opposite to velocity.

The other quantities are as previously defined. The differential equations of motion are

$$-\tau - F = Jp^2\theta = Jp\omega \quad \text{for} \quad 0 < t < t_1 \tag{36}$$

and

$$-\tau + F = Jp^2\theta = Jp\omega \quad \text{for} \quad t_1 < t < t_2. \tag{36a}$$

The known terminal conditions are

$$\left.\begin{matrix} t = 0 \\ \omega = \omega_0 \\ \theta = 0 \end{matrix}\right\}, \tag{37}$$

$$\left.\begin{matrix} t = t_1 \\ \omega = 0 \\ \theta = \theta_1 \end{matrix}\right\}, \tag{38}$$

$$\left.\begin{matrix} t = t_2 \\ \omega = \omega_2 \\ \theta = 0 \end{matrix}\right\}. \tag{39}$$

Integrating (36) and using (37) to determine the constants of integration give, for the interval $0 < t < t_1$,

$$\omega = -\alpha_1 t + \omega_0 \tag{40}$$

and

$$\theta = -\frac{\alpha_1}{2} t^2 + \omega_0 t, \tag{41}$$

where

$$\alpha_1 = \frac{\tau + F}{J}. \tag{42}$$

Using (38) in (40) to obtain t_1, and putting this value of t_1 in (41) gives

$$t_1 = \frac{\omega_0}{\alpha_1}, \qquad \theta_1 = \frac{\omega_0^2}{2\alpha_1}. \tag{43}$$

Integrating (36a) and evaluating the constants of integration by (38) and (43) give

$$\omega = -\alpha_2(t - t_1), \tag{44}$$

$$\theta = -\frac{\alpha_2}{2} t^2 + \frac{\alpha_2}{\alpha_1}\omega_0 t + \frac{\omega_0^2}{2\alpha_1} - \frac{\alpha_2\omega_0^2}{2\alpha_1}, \tag{45}$$

where

$$\alpha_2 = \frac{\tau - F}{J}.$$

(39) in (45) gives

$$t_2 = t_1\left(1 \pm \sqrt{\frac{\alpha_1}{\alpha_2}}\right), \tag{46}$$

in which the plus sign alone is of interest since $\alpha_1 > \alpha_2$. Using (46) in (40) it is found that

$$\frac{\omega_2}{\omega_0} = -\sqrt{\frac{\alpha_2}{\alpha_1}}. \tag{47}$$

Since the second half of the cycle is identical in form with the first half, the ratio of initial to final velocity will be the same in both cases. The value ω_4 of ω at $t = t_4$ is then given by

$$\frac{\omega_4}{\omega_2} = \frac{\omega_2}{\omega_0} = -\sqrt{\frac{\alpha_2}{\alpha_1}}$$

and

$$\frac{\omega_4}{\omega_0} = \frac{\alpha_2}{\alpha_1}. \tag{48}$$

Equation (48) shows that if the servo starts with a finite amplitude of oscillation, this amplitude is reduced by a constant factor each cycle, and thus the amplitude approaches zero asymptotically. In the steady-state, therefore, this ideal servo, like the previous one, oscillates with zero amplitude and infinite frequency when the input has a constant value.

Although this condition if of interest as a limiting case, the effect of time lag is always present practically. As the second condition then, the effect of a definite time lag of t_1 seconds in the reversal of the restoring torque is considered. The nature of the variations of angle, velocity, and torque with time in this condition is sketched in Fig. 7. Remembering that during each interval when the resultant torque is constant, the motion is that of a uniformly accelerated body and using the following terminal conditions,

$$\left.\begin{aligned} t &= 0 \\ \omega &= \omega_0 \\ \theta &= 0 \end{aligned}\right\}, \tag{49}$$

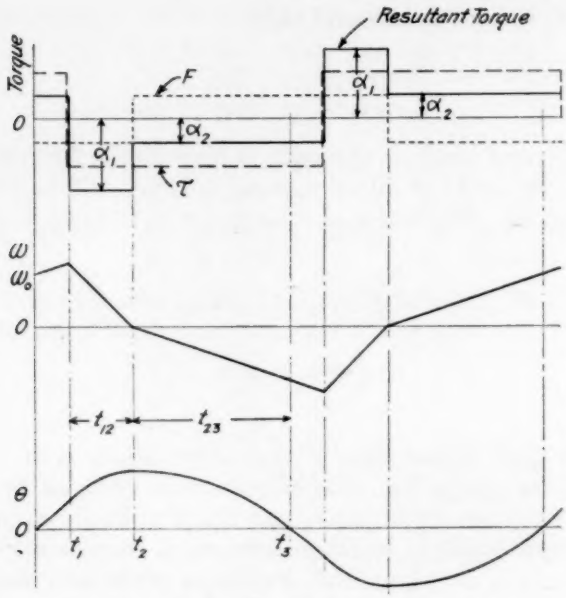
$$\left.\begin{aligned} t &= t_2 \\ \omega &= 0 \\ \theta &= \theta_2 \end{aligned}\right\}, \tag{50}$$

$$\left.\begin{aligned} t &= t_3 \\ \omega &= -\omega_0 \text{ for steady state} \\ \theta &= 0 \end{aligned}\right\}, \tag{51}$$

the following expression is obtained for θ_2 starting from the conditions at $t = 0$:

$$\theta_2 = \omega_0 t_1 + \frac{\alpha_2}{2} t_1^2 + \frac{I}{2\alpha_1}(\omega_0 + \alpha_2 t_1)^2. \tag{52}$$

FIG. 7.



Same as Fig. 6 except that time lag is present.

Using (51) and writing an expression for θ_2 starting from $t = t_3$,

$$\theta_2 = \frac{\omega_0^2}{2\alpha_2} . \tag{53}$$

Equating (52) to (53) and solving the result for ω_0 there results for steady-state operation:

$$\omega_0 = \alpha_2 t_1 \frac{\alpha_1 + \alpha_2 \pm \sqrt{2\alpha_1(\alpha_1 + \alpha_2)}}{\alpha_1 - \alpha_2} . \tag{54}$$

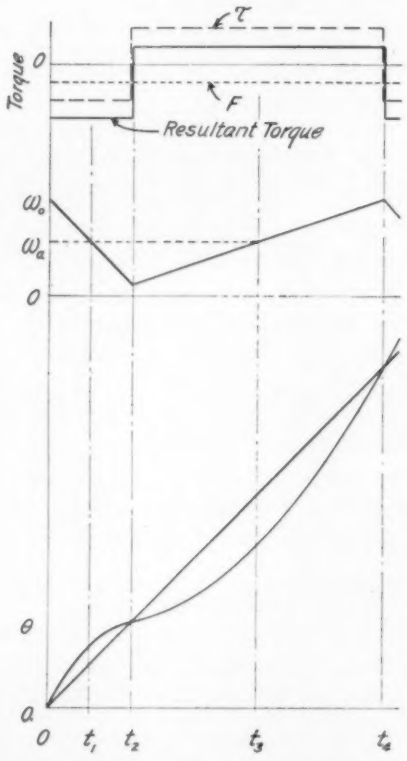
Equations (54) and (53) give the interesting result that when the friction obeys the Coulomb law, the relay servo with a stationary input oscillates with an amplitude proportional to the square of the time lag in the restoring force change. In contrast to the previous case, similar except that the damping was due to viscous friction instead of Coulomb friction, the amplitude due to a small time lag is

very small. From the point of view then of making the amplitude of oscillation small, a servo with Coulomb friction acting on the output is preferable to one with viscous friction. This statement applies, as will be seen below, only to the condition with a substantially stationary input.

The third condition is similar to the first for this case, except that the input, instead of being stationary, has a constant angular velocity ω_a.

In Fig. 8 are shown the general relations between the

FIG. 8.



Similar to Fig. 6 except that input has constant input velocity ω_a.

torques, velocities, and angles, and time for this condition. It will be assumed that the output velocity is always positive, i.e., that although the output velocity varies above and below

the average or input velocity, it never becomes negative, and that as a consequence the frictional force always acts in the same direction. The implications of this assumption become evident in the analysis.

The differential equations of motion are:

$$-\tau - F = Jp^2\theta = Jp\omega \quad \text{for} \quad 0 < t < t_2, \tag{55}$$

$$\tau - F = Jp^2\theta = Jp\omega \quad \text{for} \quad t_2 < t < t_4. \tag{56}$$

The known terminal conditions are:

$$\left.\begin{matrix} t = 0 \\ \omega = \omega_0 \\ \theta = 0 \end{matrix}\right\}, \tag{57}$$

$$\left.\begin{matrix} t = t_2 \\ \omega = \omega_2 \\ \theta = \omega_a t_2 \end{matrix}\right\}, \tag{58}$$

$$\left.\begin{matrix} t = t_4 \\ \omega = \omega_4 \\ \theta = \omega_a t \end{matrix}\right\}. \tag{59}$$

From (55) and the terminal conditions (57) the expressions for output velocity and angle for the interval $0 < t < t_2$ are found to be

$$\omega = -\alpha_1 t + \omega_0, \tag{60}$$

$$\theta = -\frac{\alpha_1}{2} t^2 + \omega_0 t \tag{61}$$

as before. From the value of θ at $t = t_2$ in (58) substituted in (61), the value of t_2 is found to be

$$t_2 = 0 \text{ or } 2\left(\frac{\omega_0 - \omega_a}{\alpha_1}\right), \tag{62}$$

the second value only being of interest. Putting this value in (60) gives

$$\omega_2 = 2\omega_a - \omega_0. \tag{63}$$

Integrating (56) and evaluating the constants by (58) and (63), the following expressions for velocity and angle during the interval $t_2 < t < t_4$ are obtained:

$$\omega = \alpha_2 t + 2\omega_a\left(1 + \frac{\alpha_2}{\alpha_1}\right) - \omega_0\left(1 + 2\frac{\alpha_2}{\alpha_1}\right), \tag{64}$$

$$\theta = \frac{\alpha_2}{2}(t^2 - t_2^2) - (\omega_a - \omega_0)\left(1 + 2\frac{\alpha_2}{\alpha_1}\right)t_2 + 2\omega_a\left(1 + \frac{\alpha_2}{\alpha_1}\right)t - \omega_0\left(1 + 2\frac{\alpha_2}{\alpha_1}\right)t. \quad (65)$$

The value of t_4 in terms of known quantities can be found by using the value of θ given in (59) in (65) and is

$$t_4 = t_2 \qquad \text{or} \qquad 2\frac{(\alpha_1 + \alpha_2)}{\alpha_1\alpha_2}(\omega_0 - \omega_a). \quad (66)$$

The second value is, of course, the only one of interest. Putting this value of t_4 in (64) gives the angular velocity at the end of the cycle in terms of the velocity at the beginning. This value is

$$\omega_4 = \omega_0. \quad (67)$$

This result is surprising at first glance for it shows that this servo, under the given conditions, continues to oscillate with any finite amplitude at which it may happen to be started. That this result is sound physically may be seen by considering the curve of resultant torque in Fig. 8. Even though friction is present, when it always acts in the same direction and is of constant magnitude, its effect is merely to increase the magnitude of the restoring force during the first portion of the cycle, and to decrease it during the second portion. Thus no damping action whatever is produced. This is the first case in which the relay-type of servo has been found to have the possibility of a finite amplitude of oscillation in steady-state operation under the ideal condition of zero time lag.

This result, of course, holds only for the postulated condition that the output velocity is unidirectional. If the amplitude of velocity oscillation is sufficiently large in comparison with the input or average output velocity so that reversal of the output velocity occurs, the action is quite different. Due to reversal of the output velocity and therefore of the direction of the friction force, a damping action is produced and the amplitude of oscillation is reduced. This reduction continues until the output velocity just fails to reverse, when the damping action ceases. Thereafter the output oscillations continue

indefinitely at such amplitude that reversal of the output velocity just fails to occur. Time lag aggravates this condition in a way that is discussed further on.

Evidently this type of servo-mechanism with constant-magnitude restoring and friction torques is unsuited for use with other than a fixed or very slowly varying input, because of the absence of useful damping when the input has appreciable velocity.

In the cases treated thus far the restoring torque has been acting at all instants either in one direction or the other. Actually there is always some inactive period which may be so small, however, in many instances that its effect is insignificant. In other instances, a finite inactive range, over which the deviation of input from output may vary without bringing the restoring torque into action, is purposely provided. The analysis of the performance of servos with such an inactive range is therefore important and will be considered next.

Case B1. In this case is considered a servo-mechanism having a constant-magnitude restoring torque and a viscous-friction torque acting on the output element.

It differs from the servo with these properties treated under A1 in that the restoring torque is not brought into action until the output differs in magnitude from the input by some predetermined angle θ_n. The analysis of this servo, while straight-forward and simple when carried out numerically for a particular case, is rather clumsy analytically because of the large number of time intervals into which a cycle of operation must be subdivided. For each of these subdivisions a different differential equation applies. Each integration to obtain an expression for angle involves two constants determined by conditions at the beginning of the time interval. Since at least five such time intervals appear in one cycle the analytical expressions become rather cumbersome by the end of the cycle.

Certain conclusions of value can be drawn without explicit formulation, however. If this servo has a stationary or slowly moving input, the inactive zone allows the servo-mechanism to remain inoperative much of the time, since the restoring torque would be called into action only when the deviation exceeded the inactive or tolerance range θ_n. It is

readily seen that under these conditions the output angle is uncertain within the range $\pm \theta_n$ from the input.

This servo will not oscillate because all of the damping forces exist that were present with no inactive zone. Consequently any initial oscillation will damp down to an amplitude that lies within the inactive zone, and then cease entirely. The effect of time lag in the application or removal of the restoring torque is discussed further on.

When the input of such a servo has an appreciable velocity, conclusions cannot be so readily drawn without a mathematical formulation, which as seen above is cumbersome. Numerical computation is perhaps as direct a method as any for obtaining an explicit result in a particular case. For a general study covering all cases, mechanical methods are the most attractive. Thus on a device such as the differential analyzer [32] many particular solutions could be run off with relative ease. These solutions could take the form of curves relating the dimensionless variables θ/θ_n and t/T, with ω_0/ω_a and ω_s/ω_a as dimensionless parameters whose values would be different for each particular solution. All possible solutions could be represented on a series of curve sheets, each sheet containing a nest of curves of θ/θ_n vs. t/T with ω_0/ω_a as a parameter, all for a given value of ω_s/ω_a. Each other sheet would be similar except that it would be for a different value of ω_s/ω_a.

The case under discussion has not sufficient interest to warrant the presentation of such a set of solutions in this paper. However, the operation of this servo-mechanism will be illustrated by a numerical example using the following values:

$$\begin{aligned} \omega_s &= 2, \\ \omega_a &= 0.5, \\ \omega_0 &= 1.5, \\ \theta_n &= 0.1, \\ T &= 1. \end{aligned}$$

Any consistent set of units can, of course, be used. The results of this calculation are plotted in Fig. 9 which shows a rather rapid damping out of oscillations. In the steady state the output oscillates with an infinitesimal amplitude

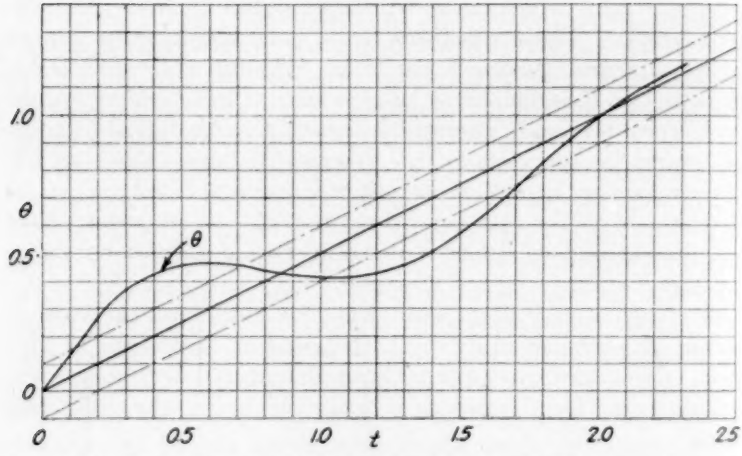
about the lagging limit, i.e., the limit at which the positive-direction restoring torque comes into action, or at an output angle

$$\theta = \theta_i - \theta_n.$$

This servo then has a definite lag angle θ_n which is half the inactive range, assuming that this range is centered on the output position for exact correspondence between input and output. From these results it appears that an inactive range has little usefulness from the theoretical point of view when the input is moving unidirectionally. Practically it may be necessary because of electrical contact difficulties or the like.

That the conclusions drawn from Fig. 9 for a constant

FIG. 9.



Curve showing output angle vs. time for a sample calculation of case B1 with constant input velocity ω_0, and no time lag.

input velocity are of rather general application can be seen from the following considerations. Suppose the initial amplitude of oscillation is very large in comparison with the width of the inactive zone. Then the analysis given under case A1 will apply approximately, leading to a smaller amplitude, when conditions will become similar to those shown in Fig. 9. If the initial amplitude is very small, the steady-state will be reached in less time than that required for the initial conditions shown in Fig. 9.

Case B2. In the case just considered the damping was due to viscous friction. In the present case Coulomb friction is assumed. The inactive zone is the same as assumed above, i.e., the output has a permissible deviation of $\pm \theta_n$ from the input before the restoring torques are effective. Except for the presence of the inactive range this case is identical with case A2.

Considering first that the input is moving slowly or not at all, a consideration of the motion of the output and the forces acting upon it will show that oscillations damp out relatively rapidly, and that the servo may remain inactive much of the time as in the foregoing case. The same uncertainty as to output position also exists.

When a constant velocity input is considered, the prediction of behavior is somewhat more involved. First assume that the initial amplitude of output oscillation is not so large as to cause reversal of the output velocity. A cycle of operation can then be represented as shown in Fig. 10. It can be shown readily that steady-state operation of this servo-mechanism can occur with any amplitude of oscillation for which the output velocity is never negative as assumed above. This is done as follows: From the differential equations and terminal conditions, or by inspection, remembering that only constant accelerations are encountered, the following relations are obtained:

$$\omega_0 - \omega_a = \omega_a - \omega_1, \tag{68}$$

$$\omega_a - \omega_2 = \omega_3 - \omega_a, \tag{69}$$

$$\omega_2 = \omega_1 - \alpha_0 t_{12}, \tag{70}$$

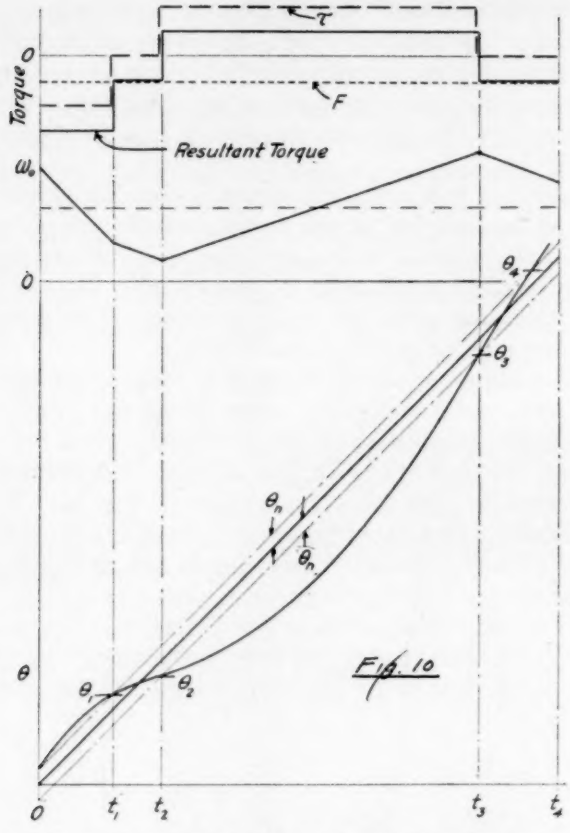
$$\theta_2 = \theta_1 + \omega_1 t_{12} - \frac{\alpha_0}{2} t_{34}^2 = \theta_1 + \omega_a t_{12} - 2\theta_n, \tag{71}$$

$$\omega_4 = \omega_3 - \alpha_0 t_{34}, \tag{72}$$

$$\theta_4 = \theta_2 + \omega_3 t_{34} - \frac{\alpha_0}{2} t_{34}^2 = \theta_3 + \omega_a t_{34} + 2\theta_n. \tag{73}$$

The significance of the subscripts is indicated in Fig. 10. Equations (68) and (69) express the fact that, in uniformly

FIG. 10.



Torques, and angular velocity and angle of output for case B2 with constant input velocity ω_0 and no time lag.

accelerated motion, the average velocity is equal to the average of the initial and final velocities. From these six equations it can be shown that

$$\omega_4 = \omega_0 \tag{74}$$

which will prove the statement made above. To demonstrate (74) substitute ω_1 from (68) in (70), substitute the resulting expression for ω_2 in (69) and solve for ω_4. This result substituted in (72) gives the following relation between ω_4 and ω_0:

$$\omega_4 = \omega_0 + \alpha_0(t_{12} - t_{34}).$$

To prove (74) it is therefore sufficient to show that

$$t_{12} = t_{34}.$$

This can be done by solving (71) and (73) for t_{12} and t_{34} respectively, and expressing ω_1 and ω_3 in the results in terms of ω_0.

It follows then that the introduction of an inactive range in the constant-friction relay servo does not damp out oscillations when the input has a constant velocity and the output velocity is unidirectional. These conditions are representative of normal operation, hence this servo is inherently unsatisfactory. To damp out oscillations under these conditions, the frictional force must have a component that varies in magnitude as some positive power of the velocity.

This completes the analysis of the various cases of relay-type servo-mechanisms considered in this paper. As this analysis has involved a considerable amount of detail, it is well to summarize the important results. This is done in Table I, which is self-explanatory except for certain of the effects of time lag discussed below. These results apply to a servo-mechanism in which the output element has inertia, and is acted upon by constant magnitude restoring forces, and a frictional force of some type.

C. Effect of Time Lag in Change of Restoring Torque.—Although in two cases, the effect of time lag in the application and removal of restoring torques was treated mathematically, a treatment of its effect in other cases was postponed for a general discussion which will be given here.

Time lag, as stated before, is the delay between (*a*) the time at which the restoring torque would be applied or removed were the relay to act and the resulting torque to build up instantaneously when the need for a change is indicated by a predetermined discrepancy, between input and output and (*b*) the time when the torque actually becomes effective. This time lag may be different for the application and the removal of the torque when there is an inactive zone, but it suffices for this discussion to consider the combined effect of these two time lags. Actually there is always some delay or time lag. In some cases it may be so small as to have an insignificant

TABLE I.

	Stationary Input.			Constant Velocity Input.		
	Steady-state Oscillation.		Average Lag Error.	Steady-state Oscillation.		Average Lag Error. No Time Lag.
	No Time Lag.	Time Lag.		No Time Lag.	Time Lag.	
A. No inactive zone 1. Viscous friction	Zero amplitude infinite frequency	Finite and function of time lag	Zero	Zero amplitude infinite frequency	Finite amplitude	Zero
2. Coulomb friction	Zero amplitude infinite frequency	Finite and proportional to square of time lag	Zero	Indeterminate	Amplitude increases until postulated conditions fail to hold	Indeterminate
B. Finite inactive zone 1. Viscous friction	None	May or may not oscillate depending on parameters	± half of inactive range with no time lag	Zero amplitude infinite frequency	Finite amplitude	± half of inactive range
2. Coulomb friction	None			Indeterminate	Amplitude increases until postulated conditions fail to hold	Indeterminate

effect upon the action of the servo. In many cases, however, its effect is significant. This is demonstrated by the great sensitiveness of the viscous-friction damped relay servo to time lag, where a small time lag results in a relatively large amplitude of steady-state oscillation. On the other hand, a relay servo having Coulomb friction is relatively insensitive to small values of time lag. These facts have been demonstrated for a servo with an approximately constant input and no inactive range.

When the input has appreciable velocity, or a finite inactive zone, the formal analysis, while straightforward, is somewhat cumbersome because of the number of points during a cycle at which the differential equation of motion changes and constants of integration must be reëvaluated. In any particular case this process can be carried through numerically with relative ease.

A qualitative analysis of the effect of time lag in these cases is readily made however. In general the result of a time lag in changing the restoring forces is to continue to increase the output velocity in a given direction after the instant at which this velocity should be decreased. Thus in an ideal relay servo with no inactive zone, if the output is ahead of the input, but is moving toward the input, the restoring torque should reverse in sign at the instant when the input and output coincide. From this instant the output is accelerated toward the input. When time lag is present, the restoring torque acting prior to this instant continues to act for the time-lag interval after this instant. During this interval the restoring torque is tending to drive the output away from the input rather than toward it. As a result, the overshoot is greater than when no time lag is present, and the relative velocity between input and output is greater when they again are in coincidence than it would have been in the absence of time lag. At the time of this second coincidence the time lag in changing the sign of the restoring torque again causes an increment of output velocity in the wrong direction. The net result, as is seen, is to tend to increase the amplitude of oscillation. Looked at in a slightly different way, the impressed torque, when time lag is present, may be resolved into two components, one with no time lag, the effect of which has

been studied in each case, and a second which gives an impulse to the output in the direction of its motion, at a time when this motion should be retarded. This impulse evidently adds energy to the oscillation, a fact demonstrated by the results in the two cases analyzed with time lag present.

The point of real interest is the effect of this increment of energy on the steady-state amplitude of oscillation. The cases previously considered are divided into two groups with respect to this effect. The first group includes the cases where positive damping exists as indicated by a zero amplitude of steady-state oscillation. The second includes the cases where no damping exists as indicated by the indefinite persistence of any existing finite amplitude.

The effect of time lag on the first group is to increase the amplitude of oscillation until the friction or damping work per cycle due to increased amplitude is equal to the work per cycle put into the output as a result of the time lag. This amplitude is finite for the cases considered in the first group.

In the second group time lag results in an amplitude of oscillation that increases until the postulated conditions of operation cease to hold. It has been seen that in the Coulomb-friction relay servo there is no damping so long as the output moves unidirectionally. When the amplitude is so large as to cause reversal of the output during some portion of the cycle, however, net work is done against friction, and positive damping is thereby introduced. Due to time lag, then, the amplitude of oscillation will increase until the work per cycle resulting from time lag is equal to the net friction work per cycle resulting from reversal of the output unless something breaks before this limit is reached. Evidently, unless the input velocity is so small that sufficient positive damping to overcome the negative damping effect of any time lag present can be introduced by a reasonable amplitude of oscillation of the output, the second group of servos is quite useless for practical purposes.

The analysis of the operation of relay servos given in the foregoing pages has assumed that the dynamic elements of the system under consideration can be reduced to an output member that has inertia, and that only frictional and constant-magnitude restoring forces act upon this member.

This is the simplest type of relay servo-mechanism but one which includes many practical cases.

This concludes the discussion of relay-type servo-mechanisms given here, which could, of course, be almost indefinitely extended. Attention will now be turned to the second, or definite-correction type.

DEFINITE-CORRECTION SERVO-MECHANISM.

Because of the relative simplicity of the operating characteristics of this type of servo-mechanism no extensive mathematical analysis will be given. The criteria for nonoscillatory response were given under the discussion of general characteristics of servo-mechanisms. Assuming that these criteria are satisfied, the other important operating characteristics are the maximum input speed which this servo can follow and its lag error. The maximum input speed is easily found.

In the definite-correction servo-mechanism, the value of the output is periodically measured to determine whether or not it differs by more than half the inactive range from the value of the input. The time interval between successive measurements is a definite quantity, independent of the manner in which the input may be varying. As a result of a measurement, a definite correction is applied to the output. If the time interval is Δt and the maximum correction that can be applied to the output in one interval is $\Delta\theta_m$, the maximum input speed ω_{im} that this servo can follow is evidently

$$\omega_{im} = \frac{\Delta\theta_m}{\Delta t}.$$

If this servo is to follow a high-speed input, either $\Delta\theta_m$ must be large or Δt small, or both. Δt is fixed by the time required for the correction to be substantially completed and for the measuring device to take up an indication substantially in accordance with the corrected value of the output. The lower limit for Δt is then established by design considerations relating to the time required for the correction to be effected, and to the time constant of the measuring device.

If only one size correction $\Delta\theta$ can be made, then $\Delta\theta$ is fixed by the permissible error, since, to prevent oscillation, $\Delta\theta$ can be no larger than twice the range of deviation of the out-

put from the input over which no correction is applied. This limitation can be mitigated by grading the size of correction, making the smallest size satisfy the error condition, and the largest satisfy the maximum input-speed condition. Of course the correction should be proportional to the measured deviation, and just sufficient to restore the deviation to zero. Nearly all definite-correction servos have this proportional correction feature.

One limitation to which this type of servo is subject should be noted. It can follow satisfactorily only those components of input velocity that are sufficiently continuous in a mathematical sense to make the input velocity approximately constant over at least two successive time intervals Δt. That is, the output varies according to a sort of step function in which the size of a given step is determined largely by the average input velocity during the preceding interval Δt. If the input velocity is sufficiently continuous, this output step function will be a good approximation to the input function. In this type of servo, however, the output always lags behind the input, because a deviation is corrected only after it has occurred. The input is always one jump ahead.

The definite-correction servo has a number of advangages for certain classes of work. When properly adjusted it is non-oscillatory. When the input is constant or varies only slowly, the output drive may be inactive for considerable periods, thus saving wear and tear. A malfunctioning of the servo is more likely to result in inaction than in a racing away of the output toward infinity or the limit stop. The relay servo is always hunting a balance position, whereas the definite-correction servo moves only when balance is disturbed. In a loose way it may be said that the relay servo keeps the output in equilibrium only by continuous juggling, while the output of the definite-correction servo is in static equilibrium. This fact makes for long life of the latter. The definite-correction servo is, however, somewhat slower than other types because of the relatively small fraction of the time during which the restoring force can be effective. This slowness imposes a restriction upon the field of application rather than upon its effectiveness in any application for which it is suitable. The definite-correction servo is widely used, especially for re-

cording and control in industrial processes where it is normally highly successful.[1, 2, 3, 9, 10, 11, 12]

CONTINUOUS-CONTROL SERVO-MECHANISM

The third type of servo-mechanism, in which the indicating element continuously controls the restoring force acting on the output element in both magnitude and direction, is perhaps the most interesting of the three. General considerations indicate that this smooth torque control should be superior to the somewhat crude "off-on" control of the relay servo. Also the continuous use of the input indications should be superior to the occasional use of these indications characteristic of the definite-correction servo. Thus the continuous-control type appears on casual inspection to have inherent advantages over the other two types, especially where accurate, rapid following is required.

The continuous-control type is interesting for another reason. Because of their nature, the forces acting in this servo are easily expressed mathematical functions of displacement and velocity. Consequently a thorough-going analysis of the performance of this servo is more readily made than of the two previous types.

The analysis given here applies to a servo in which an output member, which has inertia, is acted upon by restoring and damping torques, the restoring torque being a function of the deviation between input and output and its derivatives, and the damping torque proportional to the output velocity. Most continuous-control servos fall into this category. Especially in high-speed servos the forces obeying the Coulomb friction law are likely to be quite negligible when compared with the forces having the effect of viscous friction.

Two cases are considered in what follows. In the first, the restoring torque is proportional to the input-output deviation and the necessary damping is secured by viscous-friction-law torques. In the second, the restoring force is a linear function of the first and second time derivatives of the deviation as well as of the deviation itself. A third case, not considered in detail here because of certain practical limitations involved in its utilization in servo-mechanisms, but of interest in other similar dynamic systems, is that in which the first

time derivative of the restoring torque is a linear function of the deviation and its first two time derivatives. Minorsky [4] treats these last two cases as applied to the rudder-hull dynamic system of the ship-steering problem. The reason that the third case will not be generally useful in servo-mechanisms is that this type of control of the restoring torque relies largely on the derivatives of the deviation and these derivatives in the usual system utilizing a servo-mechanism are not sufficiently continuous to provide smooth operation of the servo. In case a servo is used in connection with a large vessel or other large inertia system, such a control of servo restoring torques might be attractive. However, the second-case type of control has not yet been exploited in servos and until this has been done that of the third case can wait. Analysis of the third case is not difficult and has been carried out to the point of determining the conditions for stability and the nature of the steady-state deviations by Minorsky for the rudder-hull dynamic system.[4] A general solution involves the solution of a cubic or higher degree equation making the results cumbersome at best. Again, however, numerical solutions for particular cases are quite straightforward.

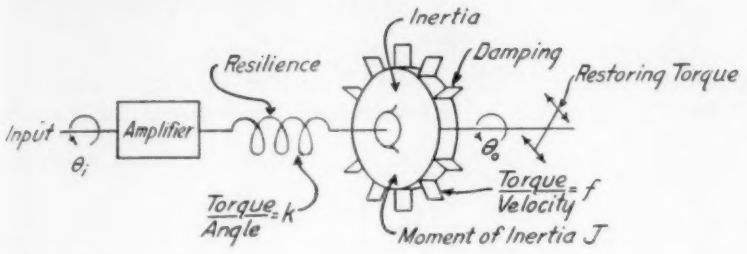
With this preliminary discussion, attention is now turned to a mathematical consideration of the first case, in which the servo restoring-force control is a linear function of the input-output deviation. Expressions for the steady-state and transient behavior are derived and a criterion of merit for such a servo is developed.

(a) Restoring Torque Proportional to Deviation.

For this analysis consider the physical system shown in Fig. 11. This system correctly represents the dynamic elements of a continuous-control servo on the condition that the effect of all inertia and damping torques can be assumed to be concentrated at the output shaft. For many continuous-control servos these assumptions correctly represent the facts.

The amplifier shown in Fig. 8 has the effect of reducing the reaction on the input of the resilience torque acting on the output by the amplification factor. Such amplification in some form is nearly always present in this type of servo. It may take electrical, mechanical or other form but the net

FIG. 11.



Schematic diagram of the significant dynamic elements of the continuous-control type servo-mechanism.

effect is the same in any case. It should be noted that the amplification factor of the amplifier affects the resilience constant, and to that extent the amplifier and resilience element are not independent as shown in Fig. 11.

In the analysis which follows a number of physical quantities not previously used appear, hence the notation for this analysis will be entirely redefined. Let

θ_i = input angle (radians),
θ_0 = output angle (radians),
$\theta = \theta_i - \theta_0$ = lag of output with respect to input (radians),
k = resilience constant (dyne-cm. per radian),
J = moment of inertia of servo referred to output shaft (gm.-cm.^{2}),
f = damping constant (dyne-cm. per radian per sec.),
t = time (seconds),
1 = Heaviside's unit function,
$p = \frac{d}{dt}$,
$\omega = p\theta$.

c.g.s. units are indicated in parentheses but of course any other consistent set is equally suitable since no empirical factors are used.

The equations governing the motion of the elements shown in Fig. 11 are:

$$k(\theta_i - \theta_0) - fp\theta_0 = Jp^2\theta_0, \tag{75}$$

$$\theta_i = \Phi(t). \tag{76}$$

$\Phi(t)$ is some arbitrary function of time representing the input motion. These equations may be solved in terms of any of the angle variables. The deviation or error angle θ is of primary interest, and from it and the given θ_i, the output angle is readily obtained. Therefore, the solution will be made in terms of θ.

Substituting

$$\theta_0 = \theta_i - \theta$$

in (75) gives

$$(k + fp + Jp^2)\theta - (fp + Jp^2)\theta_i = 0$$

or

$$\theta = \frac{f + Jp}{k + fp + Jp^2} p\theta_i. \tag{77}$$

To obtain the error angle θ as an explicit function of time some $\theta_i = \Phi(t)$ must be chosen for which the error is to be determined. An interesting case and one that imposes a severe test on a servo consists in suddenly applying a constant velocity to the input, previously at rest. Furthermore, from the response to this particular time function, the response to any $\Phi(t)$ can be determined by the use of the superposition integral.[31] If the magnitude of the suddenly-applied velocity is ω_1,

$$p\theta_i = \omega_1 1. \tag{78}$$

This expression in (77) gives

$$\theta = \frac{p + \frac{f}{J}}{p^2 + \frac{f}{J}p + \frac{k}{J}} \omega_1 1. \tag{79}$$

Equation (79) can be evaluated directly from a table of operational formulas.[31] To do this the denominator of (79) should be written in the form,

$$(p + \alpha - \beta)(p + \alpha + \beta),$$

in which

$$\alpha \pm \beta = \frac{f}{2J} \pm \sqrt{\frac{f^2}{4J^2} - \frac{k}{J}}. \tag{80}$$

Three cases of solution of (79) exist corresponding to positive, zero, and negative values of the quantity under the radical of (80). Two of these cases are of particular interest in this problem, the first or critically-damped case for which

$$\beta = 0 \tag{81}$$

and the second or oscillatory case for which

$$\begin{aligned} \beta &= j\sqrt{\frac{k}{J} - \frac{f^2}{4J^2}} \\ &= j\phi, \end{aligned} \tag{82}$$

ϕ being a real number. The third case for which β is real, corresponds physically to overdamping. This case is of relatively minor interest from the design point of view because the steady-state lag error is greater and the speed of response less than in the critically-damped case, and there are no compensating advantages in the use of over-damping.

Although (81) is a limiting case of (82), the solution of (79) using (81) is somewhat simpler than that for the oscillatory case and is therefore given first. Putting (80) and (81) in (79) there results

$$\theta = \left[\frac{p}{(p+\alpha)^2} + \frac{2\alpha}{(p+\alpha)^2}\right]\omega_1 1. \tag{83}$$

From a table of operational formulas,* the time function corresponding to (83) is found to be

$$\theta = \omega_1\left[\frac{2}{\alpha} - \left(t + \frac{2}{\alpha}\right)\epsilon^{-\alpha t}\right].$$

Substituting the time constant T for $1/\alpha$ and dividing the resulting expression by $\omega_1 T$ to obtain a dimensionless equation,

$$\frac{\theta}{T\omega_1} = 2 - \left(\frac{t}{T} + 2\right)\epsilon^{-(t/T)}. \tag{84}$$

This equation is plotted as curve 1 of Fig. 12 in terms of the dimensionless variables $\theta/T\omega_1$ and t/T. These results are discussed below in connection with those which will now be obtained.

* Ref. 31 of Bibl., Formulas (7) and (8) of Appendix C.

When ϕ is finite and real, the response is oscillatory. A solution for this case is important. Here (79) can be written in the form,

$$\theta = \frac{p + 2\alpha}{(p + \alpha - j\phi)(p + \alpha + j\phi)}\omega_1 1. \tag{85}$$

Equation (85) integrates into the form,

$$\theta = \psi_1(t) + 2\alpha\psi_2(t). \tag{86}$$

By formula * the first and second terms of (86) are found to be respectively:

$$\psi_1(t) = \frac{\omega_1}{\phi}\epsilon^{-\alpha t}\sin \phi t \tag{87}$$

and

$$\psi_2(t) = \frac{\omega_1}{\alpha^2 + \phi^2}\left[1 - \epsilon^{-\alpha t}\left\{\frac{\alpha}{\phi}\sin \phi t + \cos \phi t\right\}\right]. \tag{88}$$

The latter is obtained after some algebraic manipulation. Substituting (87) and (88) in (86), replacing $1/\alpha$ by the time constant T, and dividing by $\omega_1 T$ to make the expression dimensionless, there results

$$\frac{\theta}{\omega_1 T} = \frac{2}{1 + T^2\phi^2} \times\left[1 - \epsilon^{-(t/T)}\left\{\cos \phi t + \left(\frac{1 - T^2\phi^2}{2T\phi}\right)\sin \phi t\right\}\right] \tag{89}$$

or in polar form

$$\frac{\theta}{\omega_1 T} = \frac{2}{1 + T^2\phi^2} - \frac{1}{T\phi}\epsilon^{-(t/T)}\sin(\phi t + \xi), \tag{90}$$

where

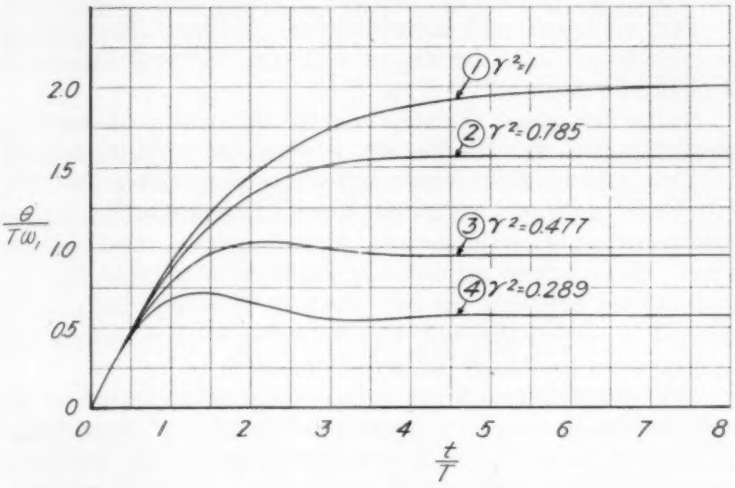
$$\xi = \arctan\frac{2T\phi}{1 - T^2\phi^2}. \tag{91}$$

In Fig. 12 (89) is plotted for various values of $T\phi$ as indicated on the curves 2, 3, and 4.

---

* Bibl. 31, formulas 17 and 11 of Appendix C.

FIG. 12.



Response of continuous-control type servo to a suddenly-applied constant input velocity. Curves of input-output deviation as a function of time with the relative damping γ^2 as a parameter. Dimensionless variables are used making these curves applicable to a servo with any constants.

Before discussing the curves of Fig. 12, it is of interest to have a single quantity which characterizes the degree of oscillation in the servo response. One such quantity, which may be called the relative damping factor, is defined by

$$\gamma^2 = \frac{f^2}{4Jk}. \tag{92}$$

From the values of T and ϕ in terms of f, k, and J it is easily shown that

$$\gamma^2 = \frac{1}{1 + T^2\phi^2}. \tag{93}$$

From the condition for critical damping, that $\phi = 0$, it will be seen that $\gamma^2 = 1$ characterizes this case, while for less than critical damping

$$\gamma^2 < 1.$$

For large values of t/T, that is when the steady-state has been reached, the substitution of (93) in (89) or (90) yields the following very simple relation between the steady state lag angle θ_s and the impressed velocity ω_1:

$$\theta_s = 2\gamma^2 T\omega_1. \tag{94}$$

The implications of equations (84), (89) and (94) will now be considered with the aid of Fig. 12. Several interesting facts can be deduced.

In the first place, the steady-state lag error is shown to be proportional to the time constant T, to the angular velocity ω_1 of the input, and to the relative damping factor γ^2. This shows that a servo with a high speed of response, i.e., a small time constant T, is inherently more accurate than a slower servo. This is true not only for the particular input function investigated here but for any form of $\theta_i = \Phi(t)$ because the factor T is carried through the evaluation of the superposition integral used to obtain the error for any $\Phi(t)$.

The significance of the relative damping factor γ^2 can perhaps best be seen by reference to Fig. 12. γ^2 characterizes the form of the deviation or error curve, that is, the amount of overshoot and persistence of oscillation. A small γ^2 corresponds to a large overshoot and subsequent oscillation. A small γ^2 also results in a small steady-state error. In designing this type of servo for small error, it is evident that some compromise must be made between the tolerated error and the amount of oscillation permitted. Fortunately the steady-state error can be very materially reduced by using a small γ^2 without introducing a very large overshoot. In certain applications, however, where the input may contain periodic components of a period comparable to that of the natural period of the servo, an aperiodic adjustment, i.e., $\gamma^2 = 1$, may be necessary.

From the design point of view perhaps the most important deduction from these equations follows from the establishment of a figure of merit for servos. Suppose this figure of merit is arbitrarily taken as the product of the numerical measure of two desirable properties of a servo-mechanism. One of these factors is taken as the maximum attainable speed ω_m. The other is taken as the smallness $1/\theta_s$ of the steady-state error θ expressed as a fraction of the total angle ω_1 turned per unit time, or ω_1/θ_s. Let the product of these two factors be the figure of merit M thus:

$$M = \omega_m \frac{\omega_1}{\theta_s}.$$

It has been seen that θ is proportional to ω in the steady-state, hence values corresponding to the maximum speed may be used and

$$M = \frac{{\omega_m}^2}{\theta_m}, \tag{95}$$

where θ_m is the steady-state error at the speed ω_m. The expression for M can be written in terms of the design constants k, f, and J as follows: From (94) using $\omega_1 = \omega_m$,

$$\frac{\omega_m}{\theta_m} = \frac{1}{2\gamma^2 T},$$

$$\frac{1}{T} = \alpha = \frac{f}{2J} = \gamma\sqrt{\frac{k}{J}}$$

and

$$M = \frac{\omega_m}{2\gamma}\sqrt{\frac{k}{J}} = \frac{k\theta_m}{4\gamma^2 J} = \frac{\tau_m}{4\gamma^2 J}, \tag{96}$$

where τ_m is the maximum torque that can be applied to the output element. Taking (95) as a measure of the desired performance of a servo-mechanism, equation (96) shows that in design, the ratio of the maximum torque τ_m to the moment of inertia J should be as large as practicable. It also shows that the relative damping should be as small as can be permitted. These are important results as they give a very definite guide for design.

In the companion paper above referred to this criterion is used as a basis for design. Tests of the servo thus designed show very fast response and an excellent agreement between test and calculation.

(b) Restoring Torque Proportional to Deviation and its First Two Time Derivatives.

The second case of the continuous-control servo will now be considered, the case in which the restoring torque is a linear function not only of the deviation θ but of its first two time derivatives. Let the restoring torque τ be given by the following relation:

$$\tau = (k + lp + mp^2)(\theta_i - \theta_0), \tag{97}$$

where l and m are constants of proportionality.

Consider the response to a suddenly-applied input velocity ω_1 just as in the first case. Using (97) in the differential equation of motion, the operational expression for the deviation or error angle θ is

$$\theta = \frac{\frac{J}{J_1}p + \frac{f}{J_1}}{p^2 + \frac{f_1}{J_1}p + \frac{k}{J_1}}\omega_1 1, \tag{98}$$

where

$$\begin{aligned} f_1 &= l + f, \\ J_1 &= m + J. \end{aligned} \tag{99}$$

Redefining α and ϕ in terms of the parameters of the denominator of (98),

$$\alpha \pm j\phi = \frac{f_1}{2J_1} \pm \sqrt{\frac{k}{J_1} - \frac{f_1^2}{4J_1^2}} \tag{100}$$

and evaluating (98) as an explicit function of time in the same way that (85) was evaluated gives the following expression for the deviation or error angle θ:

$$\theta = \frac{\omega_1 f}{J_1\alpha^2}\left[1 - \epsilon^{-\alpha t}\left\{1 + \alpha t - \frac{\alpha^2 J t}{f}\right\}\right] \tag{101}$$

for the critically-damped case, i.e., for $\phi = 0$, or

$$\theta = \frac{\omega_1 f}{J_1(\alpha^2 + \phi^2)} \times\left[1 - \epsilon^{-\alpha t}\left\{\cos\phi t - \frac{J(\alpha^2 + \phi^2) - \alpha f}{f\phi}\sin\phi t\right\}\right] \tag{102}$$

for the oscillatory case, i.e., for ϕ finite and real.

These are very interesting results for they show that the steady-state error of this servo can be made zero by making the factor f zero. f is the coefficient of damping for the viscous-friction torque acting on the output. Although this damping is made zero, the servo operation can be made aperiodic or oscillatory in any desired degree by the damping effect introduced by the component of restoring torque depending on the first derivative of θ. By this method the damping

effect is due not to the velocity $p\theta_0$ of the output but to the relative velocity $p\theta$ of the output with respect to the input. The magnitude of this damping is proportional to the coefficient l in (97). Physically the torque corresponding to the term $lp\theta$ in (97) is readily introduced, when a d-c. vacuum-tube amplifier is used, by an inductance component of interstage coupling.

The effect of introducing a component of restoring torque proportional to the relative acceleration of output and input, i.e., of introducing the effect represented by the term $mp^2\theta$ of (97) is only to alter the equivalent output inertia from the actual inertia J to an equivalent value $J_1 = J + m$. By making m negative numerically, the equivalent inertia can be given any value positive or negative. Negative values evidently are barred practically because the coefficient of the exponent in the transient-term exponential would be positive and the servo would be unstable. So long as $m > -J$, however, the system will be stable assuming of course that

$$f_1 = f + l > 0.$$

Subject to the above conditions for dynamic stability, a given servo with adjustable coefficients l and m can be made arbitrarily accurate, and fast in the sense that transient effects disappear rapidly, by giving suitable values to l and m. This is a very important result.

Physically the effect of a coefficient m can be introduced in a d-c. vacuum-tube amplifier by the use of two successive inductive interstage couplings. A negative m is secured by reversing the sense of one of these couplings. Mechanically these coefficients l and m could presumably be introduced by suitable viscous friction and inertia couplings respectively. Gyroscopic methods could also be applied to this end.[4]

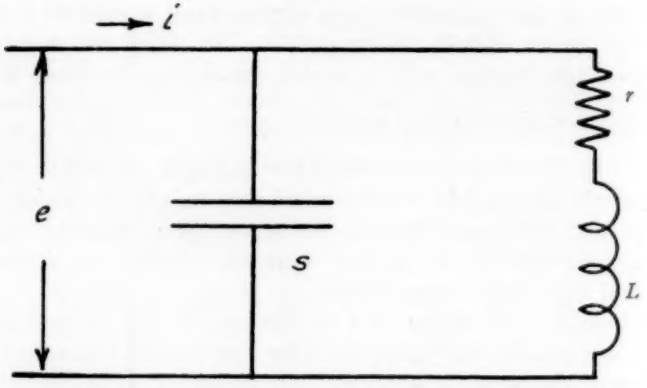
The results of the analysis of this last case open up a great opportunity for improvement in the performance of servo-mechanisms of the continuous-control type. At sufficiently high speeds of response, however, the dynamic system of a simple servo-mechanism fails to reduce to the system postulated in this analysis and the analysis no longer applies. This fact sets one limitation upon indefinite improvement. Another important practical factor is that of stability of opera-

tion which may fix a limit below which the effective inertia cannot be reduced. Nevertheless, very real gains in the performance of even present high speed response servos using deviation control should be possible by the addition of deviation derivative control.

SIMILAR DYNAMIC SYSTEMS.

Earlier in the paper it was stated that although the analyses were made in terms of rotation, they could be applied without change to a dynamically similar system involving translation, electric currents, etc. As an example of this parallelism an electric circuit which is equivalent to the continuous-control servo analyzed is shown in Fig. 13. Using

FIG. 13.



Electric circuit equivalent to continuous-control servo-mechanism.

the parallelism shown in Table II it is seen that the charge on the susceptance S is analogous to the servo error.
Thus, writing the operational expression for q_s, the charge on the condenser, there results.[31]

$$q_s = \frac{e}{S} = \frac{iZ(p)}{S}$$

$$= \frac{1}{S} \frac{(r + pL)\frac{S}{p}}{r + pL + \frac{S}{p}} i$$

TABLE II.

Quantity.	Rotation.	Translation.	Electric Circuit.
Displacement	Angle θ	Distance x	Charge q
Velocity	$p\theta$	px	Current i
Acceleration	$p^2\theta$	p^2x	pi
Force	Torque τ	Force F	e.m.f. e
Inertia	Moment of inertia J	Mass M_x	Inductance L
Resilience	Spring constant K	Spring constant K_x	Elastance $S = \frac{1}{\text{capacitance}}$
Loss constant	Viscous friction f	Viscous friction f_x	Resistance r
Time	t	t	t

$$= \frac{r + pL}{S + rp + Lp^2} i. \tag{103}$$

The resemblance of (103) to (77) is evident, remembering that $pq = i$. If the current i is taken as

$$i = II, \tag{104}$$

where I is a constant, (103) can be written

$$q_s = \frac{p + \frac{r}{L}}{p^2 + \frac{r}{L}p + \frac{S}{L}} II. \tag{105}$$

Equation (105) has solutions of exactly the same form as (84) and (89) but in terms of the electrical quantities corresponding to rotational quantities as shown in Table II.

The translational problem is strictly analogous to the rotational problem so it is unnecessary to go into details of the analogy here.

As mentioned before, the rudder-hull dynamic system involved in the automatic steering of ships is similar to the servo-mechanisms here analyzed. These examples suffice to illustrate the correspondence between analogous systems.

ANALYSIS OF OTHER SERVO-MECHANISMS.

In the foregoing analysis of servo-mechanisms, a dynamically simple system was assumed both because most servo-

mechanisms are adequately represented by this system and because this system is readily analyzed. This system consists of an output element having inertia, and acted upon by a frictional force of some type and a restoring torque which is some function of the input-output deviation.

For servo-mechanisms involving additional dynamically-significant elements, the analysis will be somewhat more complex. The differential equations of motion will, in general, be of higher order than the second and hence somewhat more lengthy algebra will be involved in their solution. The method of formulation is, however, essentially the same as that used here.

In the case of the continuous-control type of servo; the procedure is easily stated. By writing the differential equations of motion, a relation between the deviation angle θ (using rotational terms as illustrative), the input angle θ_i and the time derivative operator p of the form,

$$\theta = F(\theta_i, p),$$

can be obtained. Equations (77) and (98) are of this form. Operational methods of evaluating such expressions as explicit time functions such as (89) and (102), due to Heaviside and later workers,* are sufficiently well developed so that the process is one of routine algebra for equations resulting from lumped-parameter systems. Practically all servo-mechanisms will be included in this class. Numerous distributed parameter systems can also be treated but may involve more than algebraic work. For lumped-parameter systems the greatest difficulty is likely to be associated with the determination of the roots of higher degree algebraic equations in one variable, a cumbersome but straightforward process numerically. Such an analysis presupposes constant parameters, i.e., a linear system, an assumption widely justified particularly for the small variations which are usually of primary importance.

It should also be mentioned that entire closed-cycle control systems are dynamically similar to servo-mechanisms and their operation is investigated by the same methods. Often a

* See ref. 31 for these methods and references to other works on the subject.

closed-cycle control contains a servo-mechanism. If the time constants of the servo and of the system being controlled are widely different, it is often possible to simplify the problem considerably by analyzing the two parts separately. Otherwise the equations of the entire system must be solved as a unit.

CONCLUSIONS.

In this paper the performance of three important types of servo-mechanisms has been analyzed. These types include the relay servo, the definite-correction servo, and the continuous-control servo.

The first or relay type is always oscillatory in response to a varying input but under certain conditions the amplitude of oscillation and lag error can be made small. If unidirectional motion occurs, the presence of Coulomb friction alone will not damp out an initial amplitude of oscillation. Time lag in the application of the restoring forces tends to increase the amplitude of oscillation.

The second or definite-correction type is aperiodic in operation when properly adjusted and is quite suitable for use with slowly varying quantities. There is a slight lag error.

The third or continuous-control type is probably the best type where high-speed response and smoothness of control are required. This type can be made to have a response which is aperiodic or oscillatory with any given decrement, by suitable design and adjustment. By using the first and second derivatives of the deviation of the output from the input, as well as the deviation itself, to control the restoring force applied to the output, a very high rate of response and a very small steady-state deviation should be attainable. This type of servo has the advantage of being susceptible to rather easy and complete analysis. Tests on a high-speed of response servo of this type with deviation control alone show an excellent check with the theory. The design and test of this unusually fast servo are given in a companion paper.

ACKNOWLEDGMENTS.

The writer is indebted to Dr. V. Bush who suggested the nature and scope of this study, and who critically reviewed the manuscript. He also wishes to thank those of the Elec-

trical Engineering staff at the Massachusetts Institute of Technology who have contributed helpful suggestions.

BIBLIOGRAPHY.

1. STEIN, I. M.: "Precision Industrial Recorders and Controllers," *J. F. I.*, Vol. 209, No. 2, pp. 201–228, Feb. 1930.
2. HARRISON, T. R.: "The New Brown Potentiometer Recorder," *Rev. of Sci. Inst.*, Vol. 2, No. 10, pp. 618–625, Oct. 1931.
3. "Kent Recording and Controlling Apparatus," *Engineering* (London), Vol. 132, No. 3428, pp. 407–408, Sept. 25, 1931.
4. MINORSKY, N.: "Directional Stability of Automatically Steered Bodies," *Jour. Am. Soc. of Naval Engrs.*, Vol. 34, No. 2, pp. 280–309, May 1922. "Automatic Steering Tests," *Jour. Am. Soc. of Naval Engrs.*, Vol. 42, No. 2, pp. 285–310, May 1930.
5. SAUVAIRE-JOURDAN: "Comment On Peut Faire Gouverner Automatiquement un Navire," *La Nature* (Paris), No. 2848, pp. 355–358, April 15, 1931.
6. FERRY, E. S.: "Applied Gyro-Dynamics," John Wiley and Sons, Inc., 1932.
7. "New Compass and Path Indicator," *Shipbldg. and Shipping Rec.*, p. 684, Dec. 5, 1929.
8. "Automatic Yacht Steerer," *Sci. Amer.*, p. 238, Apr. 1933.
9. "Hydraulic Turbine Governors and Frequency Control," *Proc. N. E. L. A.*, Vol. 88, pp. 542–572, 1931. Also as Pub. No. 13. Report Hydraulic Power Committee. (Contains extensive Bibliography.)
10. MCCREA, H. A.: "Automatic Control of Frequency and Load," *G. E. Rev.*, Vol. 32, No. 6, pp. 309–313, June 1929.
11. JONES, D. M.: "Controlling Load, Maintaining Frequency," *El. Wld.*, Vol. 95, pp. 1072–1076, May 31, 1930.
12. SPORN AND MARQUIS: "Frequency, Time and Load Control on Interconnected Systems," *El. Wld.*, Vol. 99, pp. 618–624, April 2, 1932.
13. CHALMERS, T. W.: "The Automatic Stabilization of Ships," Chapman and Hall, Ltd., London, 1931. This material also appeared in *The Engineer*, London, Apr. 4 to June 27, 1930.
14. "The Gyroscopic Stabilizing Equipment of the Lloyd Sabado Liner Conte de Savoia," *The Engineer* (London), Vol. 153, I, pp. 32–35, 45, Jan. 1, 1932; II, pp. 62–65, Jan. 8, 1932.
15. FRISCH, E.: "Controlling a Gyro Stabilizer," *El. Jl.*, Vol. 29, No. 7, pp. 408–410, July 1931.
16. SCHILOVSKY, P. P.: "The Gyroscopic Stabilization of Ships," *Engineering* (London), Vol. 134, No. 3491, pp. 689–690, Dec. 9, 1932; Discussion, Vol. 135, pp. 53–54, Jan. 13, 1933; p. 107, Jan. 27, 1933.
17. VITERBO, F.: "Girostabilizzatori antirullanti (Anti-Rolling Gyro Stabilizer)," *Ingegnere*, Vol. 5, No. 3, pp. 140–152, Mar. 1931.
18. HAUS, F.: "Stabilité automatique des Avions," *L'Aéronautique*, Vol. 14, Nos. 156–159, May, June, July and August 1932. English translation, "Automatic Stability of Airplanes," N.A.C.A. Tech. Memo. No. 695, Dec. 1932.
19. GREEN AND BECKER: "Radio Aids to Air Navigation," *Elect. Engrg.*, Vol. 52, No. 5, pp. 307–312, May 1933.

20. SPERRY, E. A., JR.: "Description of the Sperry Automatic Pilot," *Aviation Eng.*, pp. 16–18, Jan. 1932.
21. HUGGINS, M.: "Gyropilot Goes Cross-Country," *Aero Digest*, Vol. 17, No. 1, pp. 51–52, July 1930.
22. LA PIERRE, C. W.: "An Improved Photoelectric Recorder," *G. E. Rev.*, Vol. 36, No. 6, pp. 271–274, June 1933.
23. BERNARDE AND LUNAS: "A Recorder for Minute Quantities," *El. Jl.*, Vol. 30, No. 3, pp. 108–109, March 1933.
"A new Electronic Recorder," *Elect. Engrg.*, Vol. 52, No. 3, pp. 168–170, March 1933.
24. GULLIKSEN, F. H.: "Recent Developments in Electronic Devices for Industrial Control," A. I. E. E., Paper No. 33–24, 1933.
25. ALFRIEND, J. V.: "Light Sensitive Process Control," A. I. E. E. Paper No. 33–45, 1933.
26. HARDY, A. C.: "A Recording Photoelectric Color Analyzer," *Jour. Opt. Soc. Am.*, Vol. 18, No. 2, pp. 96–117, Feb. 1929.
27. BAYLE, M. A.: "Un Héliostat Sans Mouvement d'Horlogerie," *Rev. d'Optique*, Vol. 10, No. 12, pp. 495–511, Dec. 1931.
28. OSBOURNE, A.: "Teaching the Robot to Think Ahead," *Chem. and Met. Engr.*, Vol. 38, No. 4, pp. 245–247, April 1931.
29. NEIMAN, *American Machinist*, Vol. 66, pp. 895–897, May 26, 1927.
30. BUSH AND HAZEN: "Integraph Solution of Differential Equations," *J. F. I.*, Vol. 204, No. 5, pp. 575–615, Nov. 1927; also as M. I. T. E. E. Dept. Bul. No. 59.
31. BUSH, V.: "Operational Circuit Analysis," John Wiley and Sons, Inc., 1929.
32. BUSH, V.: "The Differential Analyzer," *J. F. I.*, Vol. 212, No. 4, pp. 447–488, Oct. 1931.

Dicarbon Gas.—(*Ind. and Eng. Chem.*, News Edition, **12**, 261.) Most everyone is familiar with the two well-known oxides of carbon; carbon monoxide (CO) and carbon dioxide (CO_2). However, probably only chemists, and not all of them, have heard of carbon suboxide (C_3O_2). Unlike its better known cousins, the suboxide is not formed through the combustion of carbon but has as its antecedent an organic compound identified by the formula $CH_2(COOH)_2$. Notice that if two parts of water ($2H_2O$) are removed from this product known as malonic acid, C_3O_2 remains.

Carbon suboxide is a gas under ordinary conditions and A. Klemenc of Vienna with his co-workers, R. Wechsberg and G. Wagner, have succeeded in decomposing the suboxide under properly chosen conditions at 200° C. to give carbon dioxide and a hitherto unknown gas, dicarbon (C_2). Dicarbon is a carmine-red gas which rapidly polymerizes to a purple-red solid carbon. The polymerization product is readily soluble in water, yielding a red solution which shows total absorption beginning at about 4700° A. This product is still too new for much to be known about it.

C.

Bacteria-Captured Nitrogen.—(*Ind. and Eng. Chem.*, New Editions, **12**, 245.) C. A. Robak, in his Scandinavia letter, tells of an unique process for industrial fixation of atmospheric nitrogen by the aid of azotobacter or other nitrogen-assimilating bacteria. The process, patented by the Swedish chemist, G. O. W. Heijkenskjöld, first provides for the growing of the bacteria in sulfite cellulose liquor that has been treated with ground limestone or milk of lime to neutralize the free sulfur dioxide. During the period of growth, air is blown in vigorously, thus supplying the nitrogen which is stored up by the bacteria while feeding on the sugar content of the waste liquor to which some phosphorus nutrients have been added. When a sufficient amount of nitrogen has been assimilated by the azobacter, the liquid portion is separated and the nitrogenous residue used for fertilizer.

Such a process can also be combined with the production of yeast from neutralized sulfite cellulose waste liquor, the nitrogen nutrients required for the yeast fermentation being produced by the azobacter either in a separate process or concurrently with the yeast fermentation proper.

C.

THE ALL STEEL AUTOMOBILE BODY FROM CARRIAGE LINES TO STREAMLINES.*

BY

GRISCOM BETTLE,

Edward G. Budd Manufacturing Company.

Mr. Hayward, Ladies and Gentlemen:

The earliest automobile bodies were built by carriage makers with patent leather fenders.

From these small beginnings to the present Chrysler Air Flow cars, a very long step has been taken. Chrysler bodies are of the newest school of construction—built of steel reinforced with steel. The older school of design, in which both wood and steel are used, continues today with marked improvements.

We can study the contrast in these two types of body construction by a series of practical tests. These tests were designed to produce shocks as similar as possible to those ordinarily encountered. The results are best shown by the use of motion pictures in which automobiles with bodies built by the two schools of construction are subjected to equal treatment.

I noticed on page 222 of the February JOURNAL OF THE FRANKLIN INSTITUTE the article, "Bodies of Wood and Steel." I wrote to Dr. McClenahan under date of February 20th taking exception to some of the statements contained therein and informed Dr. McClenahan that we had some moving pictures which show comparative tests of the two types of body construction. Dr. McClenahan explained that the Institute does not accept responsibility for the subject matter appearing in the JOURNAL. There then followed a kind invitation to appear on this platform and refute the statements referred to above.

I would like to begin by reading from the article the following sentence, "The company advocating a combination claims

* Presented at a meeting held Thursday, March 29, 1934.

that hard wood in the body reinforces the steel, absorbs shocks and eliminates the rattle and rumble." This sentence makes three points which will be developed later in order.

The next sentence develops the fact that when the two types of bodies were subjected to a diagnoal load of 9,000 lbs., the all-steel body was wrenched 11 inches out of line while the composite body was deflected only 4 inches. Ladies and gentlemen, I would answer that by the statement that that is of academic interest only, as automobile bodies are not designed to withstand any such freakish loads nor is there any occasion for them to do so in normal use.

The claim that hard wood reinforces the steel in an automobile body is generally substantiated by this little sample which you have doubtless seen at every automobile show for years. This sample conclusively proves that a metal tube filled with wood is stronger than the sum of both of them tested separately. This again is of academic interest only, for there is no place in an automobile body where anything such as this rod and tube exists.

The claim that wood absorbs shocks will be passed over here as that will be fully developed in the moving pictures which you will see a little later and where it is subjected to severe shock.

The claim that wood is a deadener of sound and eliminates rattle and rumble is true—and yet untrue. There is nothing inherent in wood which gives it sound-dampening properties, or else why would Stradivarius have used it in his violins or Steinway in his pianos? Wood, or any other material, when placed next to a vibrating panel has a dampening effect similar to touching a tuning fork with the finger. This can better be accomplished, however, by the methods employed in the all-steel body, namely, to stretch and form the panels—in this way eliminating flat drumming sections (Fig. 1) and by applying some soft material to the panels as is done in this exhibit (Fig. 2).

I am reminded of a story of a loving couple watching over their baby in his crib, the father sitting on one side and the mother on the other. Through the mother's dreams, the future of her wonderful offspring looms. She is sure that when he goes to kindergarten, he will be the brightest kiddie

in the class. Then when he goes on to school, he will be the most popular and best looking boy, captain of the football team and lead his class in studies. When he goes to college, she visualizes his becoming an All-American tackle with a

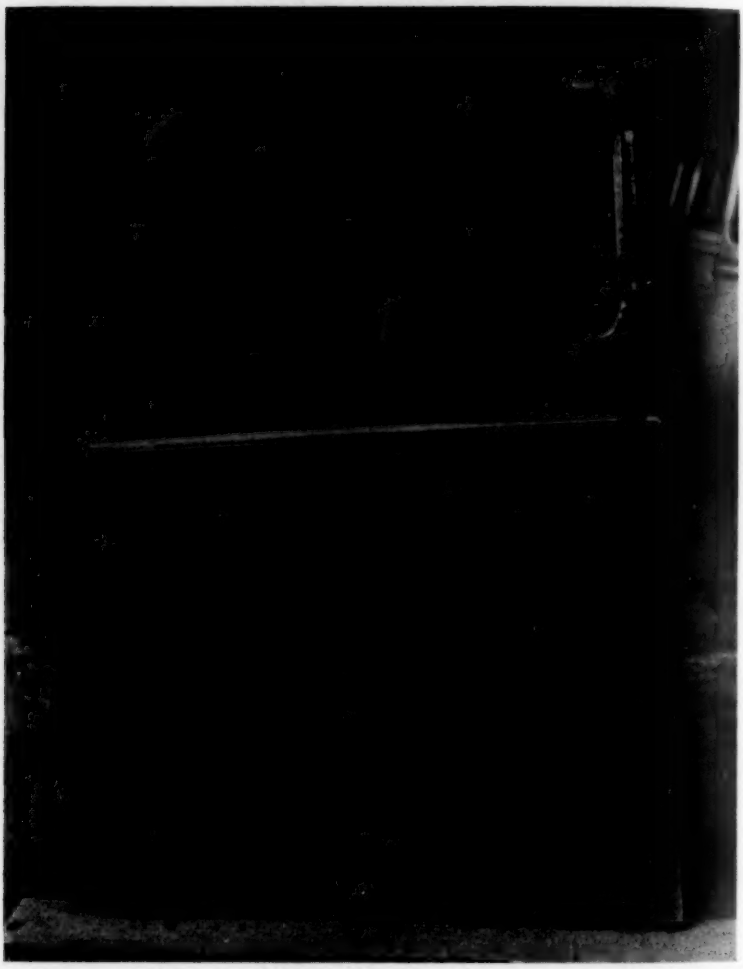
FIG. 1.

Dr. Winlock, head of the Budd Metallurgical Laboratory demonstrating a flat sheet of steel and a piece of the same size which has gained much rigidity and strength and has lost much sound-giving properties by virtue of its having been formed in a press.

Phi Beta Kappa key on his watch chain. He then will proceed by easy stages of preferment to the governorship of his state. As United States Senator he will make a name for himself and finally as President of these United States he

will lead the country to better things. At that moment she looks across the crib at her husband and says, "A penny for your thoughts, my dear." He wakes up rather startled and

FIG. 2.



Illustrating the application of a sound deadening moisture-proof pad to the inside of a door panel.

says, "I was wondering how in hell they make that crib for $2.95." Since I have been connected with this great industry, I have often wondered, like this father, how they make a

car for such a small price and I will attempt to give you a little background of the history of the development of an important phase of that industry—the automobile body.

In the 90's when the automobile first came into being, the passenger-carrying part of the vehicle, the so-called bodies, were naturally patterned after the then existing art of the carriage builder. Carriages in those days were made from a framework of wood on which thin panels of wood veneer were nailed to keep out the weather and to carry the paint.

At the turn of the century, as the automobile became a vehicle which could obtain speeds of 25 or 30 miles an hour and really became of useful service, pure coach construction was no longer adequate and so the wood veneer panels were replaced by panels of light gauge steel. These panels were more substantial and better carried the paint, giving a far greater resistance to the weather and the abrasive action of dust and stones which flew up in great profusion as one travelled the gravel paths of the country, in the early days of 1900.

The automobile industry has always been America's most ingenious and progressive business, so that we were not long content with this coach construction, and the flat tinny panels of the early days soon were replaced by curved stampings. Engineers began to give contour and a more pleasing appearance to the automobile. However, we coasted along for a number of years making continuous improvements but no very radical change, because in those days the vast majority of the automobiles sold were open cars with a cloth top for protection against the elements. It wasn't until after the war that the modern closed body took its rightful place in the popularity of the public.

During this transition, however, little was done to improve the basic construction of the body as, with one or two exceptions, bodies were generally made from a wooden framework on to which were nailed panels of steel, and this construction, based on the coach builder's art is still in use today. The great difficulty with the so-called composite body lies in the fact that wood and steel do not mix in a structure, as no one has ever yet found a satisfactory method of permanently joining wood and steel. Wood and steel are

subject to different laws of expansion and contraction and different periods of deterioration, which tend with vibration and age to loosen the nails and screws by which the two are joined. Consequently, a composite construction of wood and steel is no stronger than its joints. On the contrary, when steel is reinforced with steel, and when the structure is electrically welded into one homogeneous whole, you will have a body which is entirely built of the same material and in which there are no loose joints to deteriorate and squeak or rattle.

In 1920 and 1921, the Budd Company built the first closed all-steel bodies. These all-steel bodies, however, were built on the same principle as the composite bodies of that day. By that I mean where a wooden post was indicated in a composite body, in the steel body a Chinese copy of that same wooden post was introduced. The great difference, however, between the steel body of those days and the composite body was due to the fact that the outside panels could be successfully welded to the steel framework, which gave a permanent joint, and body engineers no longer had to rely on wood screws and nails and glue to hold their structure together.

These early bodies, although a big step in the right direction, were not entirely satisfactory because they were made from 800 or 900 small pieces welded together. This made for manufacturing difficulties and also made for many joints.

In the early 20's the steel body therefore was decidedly along the right direction but had not arrived as yet as a popular favorite with the public. It was generally used on low-priced automobiles. This was not so much due to any inherent objections to all-steel bodies as to the large tool costs required to produce an all-steel body. The reason, of course, was that it was only the manufacturer of low-priced cars which sold in large volume who could afford the necessary tools to produce an all-steel body.

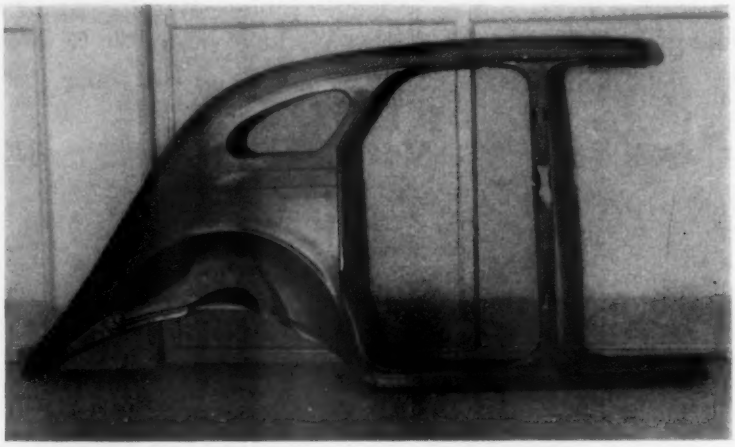
Automotive engineers watched closely the pioneering of the all-steel body and recognized its merits. We set about to accomplish two things. First, to overcome the popular objection to the steel body, which in the early days revolved itself around noise. Secondly, to use to the fullest extent the inherent characteristics of steel, which is recognized as the best and most economical building material known to man.

The first objection was solved entirely to our satisfaction and to the satisfaction of the public by putting greater formation and stretch into the panels, thus avoiding flat drumhead vibrating surfaces, and secondly by applying to the inside walls a pad of sound deadening material, very much as was done by engineers in the planetarium dome.

The second point, the full utilization of steel, was accomplished by making the stampings larger and fewer and by employing the new art of flash welding to weld these pieces into one continuous whole. The result was the so-called monopiece body (Fig. 3).

All of the above speaks of our alertness in working out for the American people a continuously improved product, and

FIG. 3.



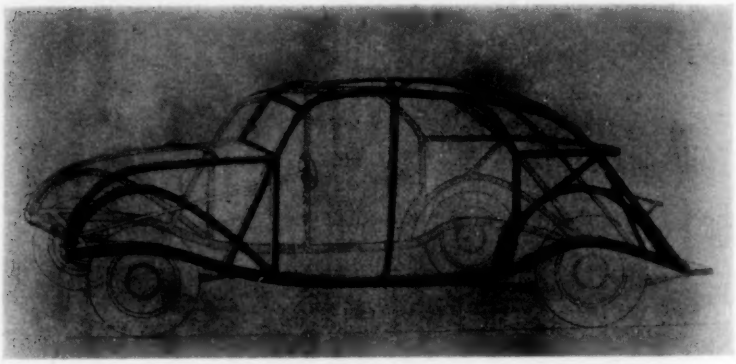
Rear Quarter Panel of Chrysler Air Flow body—one of the largest and most difficult stampings ever made.

the results in general public acceptance have justified the end. But we were not satisfied with present accomplishemnts, and when the Chrysler body engineers set about to develop the 1934 air flow, they realized that it could only be done by throwing all traditions to the winds and forgetting Napoleon's coach, the milk wagon and the buggy era. They must start from the ground up. Out of everything that had gone before, they turned to *steel* reinforced with *steel* to produce a homo-

geneous structure of proven worth. Most automobiles have been constructed of a chassis with a rigid frame, on which was set a body, as a howdah is strapped to an elephant's back. In a howdah the occupants ride to be protected from the jungle tiger, but beyond that all the howdah does is to increase the load on the elephant's back.

Now I want to point out that in a perfect engineering structure every part should have a duty and that all component parts should be so designed that they perform a useful work. Consequently, on the 1934 air flow automobiles, Chrysler decided actually to make the body do some work, and thereby to increase the strength of the structure and at the same time to lighten the total weight. They, therefore, built into the body trusses like the sides of a bridge, and have put the passengers down in the framework so that they now ride in a position similar to comfortable seats placed on the floor of a steel bridge (Fig. 4). We have been able to throw

FIG. 4.



Sketch illustrating truss-like steel Reinforcements which carry a large part of the load in a Chrysler Air Flow body. It is obvious that these could not be satisfactorily made from wood.

away not only the old wooden framework, but we have been able to throw away a part of the weight formerly employed in the massive chassis frame and have been able to produce a lighter and stronger automobile and one which, partly on account of its lesser weight, gives brilliant performance and economy.

I wish to quote from an article by Oliver Clark, Chief Body Engineer of Chrysler: "I wish to again reiterate that without that splendid material, steel, this could not have been accomplished. It seems logical to me that a material which for years has proven itself to be the best material with which to build railroads, bridges, skyscrapers, aeroplanes and ships, and has proven itself as the material from which the safest, strongest, longest lived automobile bodies have been built is the right material from which to make the new 1934 Chrysler Airflow bodies. Without the use of 'steel reinforced with steel,' this car could not have been made."

In closing I am glad to state that the present trend to streamlining and its consequent complicated contours is bound to settle for good and all the old controversy between the composite and all-steel body, for bodies of the airflow type can only be made from "steel reinforced with steel."

Shrink Fits.—Among the many interesting 1934 Preprints issued by the American Society for Testing Materials is one on the "Frictional Resistance of Steel and Brass in Shrink Fits." One notable feature about this investigation by W. H. Sevanger is that the shrink fits were accomplished by refrigerating or contracting the inner member rather than adopting the usual procedure of heating and expanding the external component. The assemblies consisted of pins, approximately one inch in diameter, set inside cylindrical rings having bores not quite so large as the pins.

Assemblage was accomplished by first immersing the pins in a refrigerating mixture of solid carbon dioxide (dry ice) and acetone. In this way, the temperature of the pins was reduced to $-112°$ F. whereby a pin, 14/10,000 of an inch oversize at ordinary temperature (68° F.), now gave a clearance of 5/10,000 of an inch when assembled, three types of assemblages were studied: a brass ring over a brass pin; a steel ring over a brass pin; and a steel ring over a steel pin. After the pins had returned to ordinary temperatures the forces necessary to strip the rings from the pins were determined with a hydraulic tension testing machine. When brass was fitted to brass, no seizing took place and the resistance to slip amounted to 1430 pounds per square inch of contact area. Some seizing evidently accurred between the steel ring and brass pin because the maximum slip resistance was 2100 pounds per square inch and the brass pin was distinctly scored with steel on steel the maximum resistance reached 6050 pounds per square inch. Severe seizing had taken place and portions of metal had been torn bodily from the pin where the adhesive force between the contact surfaces exceeded the cohesive strength of the steel.

These experiments indicate that in shrink fits made by refrigerating the inner member, seizing does not occur until relative motion has taken place between the two contacting surfaces. The force necessary to cause initial slip is probably largely dependent upon the amount of "over-size" of the inner member. After seizing has occurred, the force necessary to continue the separation of the fitted part is very much increased.

C.

THE PHYSICAL CAUSE BACK OF THE RELATIVITY EQUATIONS.

BY

JAMES MACKAYE, B.S.

The character of the explanations provided by the theory of relativity is known to be something of a puzzle to physicists. Some claim that the theory affords *solutions*, *explanations* and *predictions*. Others that it does not. Weyl for instance says that the theory "solves the perplexing problem of gravitation and of the relativity of motion at one stroke in a manner highly satisfying to our reason," [1] whereas Bridgman says that "the explanatory aspect is completely absent from Einstein's work," [2] and suggests that an attempt be made to ascertain how it happens that equations without explanatory aspect turn out to be explanatory. Thus he remarks:

"Three definite conclusions about the physical universe have been taken out of the hat by the conjuror Einstein . . ., and the problem for us as physicists is to discover by what process these results were obtained." [3]

Perhaps a clue to the mystery may be found if a parallel though simpler example of a process for conjuring definite conclusions about the physical universe out of the hat is presented.

If a small aperture is made near the bottom of a cask of water, the water will run out through it, and, other things being equal, the deeper the water, i.e., the greater the "head," the faster it will run. Torricelli worked out the law for the dependence of velocity of efflux on head, and found that if h is the head, g the gravitational constant, and v the velocity of efflux, then $v = \sqrt{2gh}$; and this law is obviously the expression of the operation of a causal agency—hydraulic pressure due to gravity. This is the physical cause back of the equation. It is the physical explanation of the fact that

[1] Weyl, H., "Space—Time—Matter," p. 247.

[2] Bridgman, P. W., "The Logic of Modern Physics," p. 165.

[3] Bridgman, P. W., "The Logic of Modern Physics," p. 172.

the velocity of efflux v is dependent for its magnitude upon, i.e., is *relative to*, the head h.

But suppose I wish to attribute the result, not to a physical cause, but simply to a relativity of one magnitude with another, say to a relativity of velocity with head, or rather length, since the head is measured in units of length. I have only to use the expression $\sqrt{2g/h}$ as a multiplier for the magnitude h—a variable one because it contains the variable h—and I shall have expressed Torricelli's law as due simply to a relativity of velocity with length. The velocity of efflux is given merely by multiplying h by this variable multiplier, much as we obtain a length measured in feet from one measured in yards by use of the multiplier or unit-changer 3. We may call such a variable multiplier, which provides us with a variable unit, a non-Newtonian unit-changer, to distinguish it from a Newtonian unit-changer such as 3, which provides us with constant units only. Thus by specifying the proper unit in which to measure the head of a cask of water, I can arrive at the expression of Torricelli's law minus its physical explanation, and can call this the relativity "interpretation" of the law. According to this interpretation hydraulic pressure or any other physical explanation may be dispensed with. The observed phenomena of efflux result from nothing more than a relativity of velocity with length expressed by the unit-changer $\sqrt{2g/h}$, which can neither be questioned nor further analyzed because it follows from the definition of the unit. Indeed, it reduces the law to a truism such as 3 feet make a yard. It is simply the *nature* of velocity of efflux to be relative to length, just as, according to the postulates of relativity, it is the *nature* of time and length to be relative to velocity, and when conclusions follow from the nature of things, other premises for them may be dispensed with.

This peculiar kind of explanation, which attributes laws of nature to "relativities" of some dimension or dimensions, and is indifferent to hypotheses about them, may be called a dimensional explanation, and we have just illustrated how one may be obtained for Torricelli's law, and thus conceal or disguise the law in the unit of measurement. By such a process we can derive the law from the unit because we have first derived the unit from the law.

That the Einsteinian explanations and predictions are of this peculiar dimensional kind, which disguise laws of nature in variable units of measurement is here put forward as an hypothesis. Space does not permit a full discussion of it, but the following citations suggest the confirmatory evidence available.

Eddington tells us that:

"The symmetry and homogeneity expressed by Einstein's law [of gravitation] is not a property of the external world, but a property of the operation of measurement." [4]

He also informs us that:

"The relativity theory is indifferent to hypotheses as to the nature of gravitation, just as it is indifferent to hypotheses as to matter and light. We do not in these days seek to explain the behaviour of natural forces in terms of a mechanical model. . . . We have to accept some mathematical expression as an axiomatic property which cannot be further analysed." [5]

And Russell says:

"This law [of the conservation of mass] . . . is to be regarded as a truism, of the nature of the "law" that there are three feet to a yard; it results from our methods of measurement." [6]

Einstein agrees with these authors that laws of nature may be discovered by mathematical constructions alone, for he tells us:

"It is my conviction that pure mathematical construction enables us to discover the concepts and the laws connecting them which give us the key to the understanding of the phenomena of Nature." [7]

Note also that Eddington, by means of the mathematical expedient used by the theory of relativity, is able to express Einstein's law of gravitation as follows:

"*The radius of spherical curvature of every three dimensional section of the world, cut in any direction at any point of empty space, is always the same constant length.*" [8]

---

[4] Eddington, A. S., "The Mathematical Theory of Relativity," p. 154.

[5] Eddington, A. S., "Report on the Relativity Theory of Gravitation," p. 91.

[6] Russell, Bertrand, "The A B C of Relativity," p. 221.

[7] Einstein, A., "On the Method of Theoretical Physics" (Herbert Spencer Lecture), Philosophy of Science, Vol. I, p. 165.

[8] Eddington, A. S., "The Nature of the Physical World," p. 139.

And then note Eddington's explanation of why we should not feel surprise at this new extension of Newton's law:

"When we felt surprise at finding as a law of Nature that the directed radius of curvature was the same for all positions and directions, we did not realise that our unit of length had already made itself a constant fraction of the directed radius. The whole thing is a vicious circle. The law of gravitation is—a put-up job." [9]

These citations prove that the theory of relativity dispenses with physical hypotheses, and employs variable units of measurement which express laws of nature as truisms. Do any explanations other than dimensional ones have these characteristics? Obviously not. Further evidence for our hypothesis is given at the close of Section 11 of Einstein's book "Relativity" where, by means of the Lorentz transformation the apparently surprising proposition is proved that the relative velocity of material bodies and light is constant, irrespective of the velocity of said bodies relative to one another, for Einstein says of his proved conclusion:

"Of course this is not surprising, since the equations of the Lorentz transformation were derived conformably to this point of view." [10]

By the phrase "conformably to this point of view" Einstein means conformably to the stipulation or postulate which he lays down in Section 8, which makes the velocity of light constant by definition—and assuredly we should not be surprised that a velocity which is defined to be constant is constant. It would be surprising indeed if it were not. These quotations make it plain that the job which Eddington tells us is put up on a law of nature is in reality put up on the unit or units of measurement in which the law is expressed, and back of units we cannot go. That would be to question the definition of the dimension itself, and of course a definition is true—by definition. It is in fact that most indisputable of truisms, an identity, subject and predicate having the same meaning.

That relativity explanations, like other dimensional ones, are "pure mathematical constructions" of the "vicious circle"

[9] Ibid., p. 143.

[10] Einstein, A., "Relativity," p. 34.

variety, which take on the guise of truisms because of the units employed, is elsewhere expressed by Eddington as follows:

"The field laws—conservation of energy, mass, momentum . . .—are not controlling laws. They are truisms. . . .

"Granting that the identification is correct, *these laws are mathematical identities*. Violation of them is unthinkable." [11]

And Russell expresses the same idea:

"The net result of relativity theory is to show that the traditional laws of physics, rightly understood, tell us nothing about the course of nature, being rather of the nature of logical truisms." [12]

It is worth while perhaps to pause a moment and consider what this claim if accepted really means. It means, does it not, that Lavoisier, Mayer, Joule, and hundreds of other investigators, were wasting their time when they tried to prove the truth of these laws by observation and experiment. Such a procedure, all unknown to them, was as superfluous as trying to prove by experiment a mathematical identity, such as that a triangle has three sides or that there are three feet in a yard. Labors like theirs can "tell us nothing about the course of nature." Can such a claim be admitted? I think not. The claim is made because Eddington and Russell, like other relativists, have not directed their attention to the distinction between dimensional and non-dimensional explanations, and hence assume that laws of nature can be magically taken "out of the hat" of mathematical identities. When this distinction is taken into account, the puzzle which Bridgman stresses is on the way to solution, for back of the equations which express the identities there is always a physical cause. The relativists of course are entirely correct in saying that their laws result from methods of measurement; and this is why Bridgman calls Einstein a conjuror. But it will be noted that by means of the unit-changer $\sqrt{2g/h}$ it is possible to make Torricelli's law result from a method of measurement. This method, however, is itself derived from experiment, and we shall at the close of this paper indicate that the relativist methods have an experimental origin also.

[11] Eddington, A. S., "The Nature of the Physical World," pp. 236, 237.

[12] Russell, Bertrand, Ency. Brit., 14th ed., Vol. 19, p. 100.

The experimentally confirmed laws are derived from methods of measurement to be sure, but not until the methods of measurement have themselves been derived from experimentally ascertained laws. And this is where Eddington's "vicious circle" comes from.

Despite the fact that the conclusions of relativity are alleged to be truisms, violation of which is unthinkable, the relativists, inconsistently perhaps, lay great stress upon their confirmation by experiment. The six principal confirmations of the special theory, to which in this presentation we will confine attention, are as follows:

(1) The Doppler effect in light.
(2) The aberration of light.
(3) The Fizeau effect.
(4) The Airy effect.
(5) The result of the Kaufmann experiment revealing the dependence of the inertia of material bodies upon velocity.
(6) The result of the Michelson-Morley experiment revealing the dependence of the configuration of material bodies upon velocity.

There are two or three others, but they are not much stressed by the relativists and space does not permit their discussion here.

That the predictions of the special theory of relativity agree with the behavior of nature as far as the above effects are concerned cannot be questioned. Hence, as Einstein maintains (referring to the Fizeau effect), the theory is "most elegantly confirmed by experiment" [13] as much so by any one of these effects as by any other, and no more so by one than by another. From this it follows that the special theory of relativity could have been propounded and confirmed by the facts long before the Michelson-Morley experiment, to which it owed its origin, was performed. The four first named effects would have confirmed it eighty years ago as they do to-day, for all these phenomena were known at that time. Indeed, when Bradley discovered the fact of aberration in 1728, Einstein's theory would have received undoubted confirmation, since the physical cause which gave rise to Bradley's

[13] Einstein, A., "Relativity," Section 13.

discovery could have been expressed in units of measurement in his time just as well as to-day, had it occurred to any of his mathematical contemporaries to devise a dimensional explanation to duplicate his (Bradley's) own.

Having ascertained this much about the verification of the (special) theory of relativity, and recalling our hypothesis that the theory provides only a dimensional explanation, let us briefly consider another relation of dimensional to physical explanations in general, with a view to discovering, if possible, what physical explanation the relativity units are really expressing. For this purpose consider the parallel case, already cited, of the law of Torricelli.

The statement that the dimensional explanation of Torricelli's law is confirmed by experiment is the statement that the rate of efflux of water under a given head h, will be observed to be that expressed by the unit-changing equation specified; and the statement will be true. The velocity of flow will be due to a relativity of velocity with length as the explanation maintains.

But now let us turn to the physical explanation—the kind to which classical physicists appeal. Would this be verified also, or would it be refuted? Classical explanations postulate no peculiar non-Newtonian "relativities" of one dimension with another. By them Torricelli's law is attributed to a varying pressure due to a varying head of water, which forces out the water through the aperture at a varying velocity of efflux. Classical physicists maintain that it is both possible and useful to go back of the unit-changer, $\sqrt{2g/h}$, to a physical explanation of the law which it expresses. Questioning the dictum of Eddington that "we have to accept some mathematical expression as an axiomatic property which cannot be further analysed" they seek causal explanations for such expressions, just as chemists and geologists and biologists do, and, like them, are often successful in finding what they seek. They are not ready to admit that physics has been forced out of step with all the rest of science, and must throw up the sponge in its efforts to find physical explanations of phenomena. At any rate it is clear that the effect must be admitted even if the causal explanation is denied, since the effect is a matter of observation. But if

the effect is due exclusively to the cause to which classical physicists appeal, then the dimensional explanation, attributing the effect to a "relativity" of velocity with length is a mere duplicate and disguise therefor, which follows from the variable unit employed. It is a simple (and true) statement of the fact that the velocity depends upon the head, and in no way opposes the classical explanation, which certainly does not deny this dependence. If, on the other hand, this relativity expresses some newly discovered fact or principle about the *nature* of velocity of efflux, some novel determinant of effects different from those to which classical physicists appeal, then the velocity of efflux expressed by Torricelli's law ought to be affected by *both* determinants, and hence both would have to be allowed for. But as both allow for the same variation with length, the correction would have to be twice that found by experiment to be called for, and this would prevent the equation from agreeing with experiment. If the two explanations are assumed to be different then, one of them must dispense with the other, and thus the acceptance of the relativity explanation involves the denial of the non-relativity one. From this it follows that we can discover what a dimensional explanation is disguising by discovering what physical explanation it *denies*. By ascertaining the cause dispensed with by the variable unit-changer we ascertain the physical agency for which the "relativity" it expresses is a substitute.

Now Einstein's theory only differs from the classical one when phenomena are observed in a frame of reference which is in motion relative to the observer who measures them. The rate of this motion may be expressed by v and the velocity of light by c. The theory stipulates that all measurements of length and time intervals, as well as all other phenomena, in the moving system shall be made by means of signals transmitted to the observer by means of light. It also stipulates that the unit-changer for conversion of Newtonian into non-Newtonian units of time, and of length parallel to the direction of motion of the moving system, shall be $\sqrt{c^2/(c^2 - v^2)}$. This is the counterpart of the unit-changer $\sqrt{2g/h}$ which was used to express Torricelli's law in a unit of length. It is usually expressed by the relativists in the more

familiar form $1/\sqrt{1-v^2/c^2}$. If the velocity of the agency which transmits the signals (c) were to become infinite it is obvious that this unit-changer would reduce to unity, and measurements made by means of signals transmitted by such an agency would be Newtonian ones. They would agree with measurements on a stationary system, a system where $v = 0$. Thus the new Einsteinian units are the Newtonian units multiplied by the reciprocal of the cosine of the aberrational angle, and on these two variable units the whole special theory of relativity rests. From this it follows that all magnitudes involving time and length—velocity, momentum, energy, etc.—become relative to velocity; which is no more than another way of saying that, if measured by an observer in the manner specified, their numerical magnitude will depend upon the aberrational angle in the manner expressed by the unit-changer—a fact which follows from ordinary classical reasoning. And the relativists use no other reasoning in establishing it. With the clue thus furnished, let us attempt to ascertain what the physical agency is whose operation the theory of relativity *denies*.

If we go back to Einstein's description of his method of obtaining his definition of "simultaneity," a definition, from which, as he shows, his variable units are derived, we may note that he postulates the principle of the constancy of velocity of light relative to all material bodies irrespective of their own relative motion. That is the one unique and novel condition which all transformations expressive of the special theory are required to meet. All other conditions are entirely compatible with non-relativity or classical assumptions. Now what physical agency does this constancy of velocity of light deny? A little consideration will show that it denies the phenomena known as the Doppler effect and the aberration of light, both of which are displacements of light caused by the motion of matter. Nor are these phenomena confined to visible light. They are characteristic of all radiation. Moreover, they are physical agencies capable of producing effects. When Roemer in 1675 discovered that light had a finite velocity he discovered a fact of which the theory of relativity, and the physical agency which its units disguise, are joint consequences. For it follows from that discovery that dur-

ing the time that light is moving from one point to another in space, material bodies can also move from one point to another in space, and hence that the velocity of material bodies relative to light waves is *not* a constant, but depends upon the motion of bodies relative to the medium in which the light moves. It is only relative to the ethereal medium that the velocity of light is constant. Previous to Roemer's discovery it was supposed that the velocity of light was infinite, in which case of course, aberration and Doppler-displacements would be impossible, for, relative to an infinite velocity, all finite velocities of bodies must be the same. Moreover, no velocity can exceed an infinite one. By the use of Einstein's units in fact the velocity of light, though finite, is made to take on these two characteristics of an infinite velocity. Thus 186,000 miles a second is the Einsteinian homologue of a Newtonian infinite velocity.

Now Doppler-displacements, constituting change (shortening or lengthening) of wave-length in light waves, and aberration, constituting change in the angle of impingement thereof, are caused by the motion of bodies relative to emitted or in-falling waves at some velocity which *differs* from c. In the absence of any difference it is obvious that neither of these effects would be possible. Moreover, the two phenomena, though apparently so different, are really the same physical agency. Aberration is simply the form which the Doppler effect assumes when the angle between the path of the moving body and that of the light departs from zero, becoming a maximum when these two paths are at right angles, when of course the Doppler effect becomes zero. In the Michelson-Morley experiment, for instance, the two effects replace each other when the interferometer is rotated, the one waxing in proportion as the other wanes. Einstein's postulate of the constancy of velocity of light therefore denies the possibility of Doppler-displacements and aberration, but as aberration is not really a separate physical agency, we may for brevity say that it denies the possibility of Doppler-displacements. *From which it may be concluded that such displacements constitute the physical agency which the variable units of the special theory of relativity are disguising and duplicating. It is this agency whose operation they really express.*

That the existence of Doppler-displacements is denied by the theory of relativity is not only demonstrable from Einstein's primary postulate of constancy of light velocity, but is explicitly contended by the relativists themselves. Note for example what Dingle has to say on this subject. After an unusually clear exposition of the classical explanation of Doppler-displacements as caused by variation in the relative motion of matter and light, he goes on to say:

"This argument, which is apparently irrefutable, is nevertheless a false one; that is to say the physical ideas it gives cannot represent reality. . . . The whole matter receives a satisfactory explanation in the light of the theory of relativity, but it would take us too far afield to introduce that subject here." [14]

For another denial notice what Eddington says about Doppler-displacements:

"It has often been suggested that the stars will be retarded by the back-pressure of their own radiation. The idea is that since the star is moving forward the emitted radiation is rather heaped up in front of it and thinned out behind. Since radiation exerts pressure the pressure will be stronger on the front surface than on the rear. Therefore there is a force retarding the star tending to bring it gradually to rest. . . .

"But according to the theory of relativity 'coming to rest' has no meaning. A decrease of velocity relative to one frame is an increase relative to another frame. There is no absolute velocity and no absolute rest for the star to come to. The suggestion may therefore be at once dismissed as fallacious." [15]

The heaping up in front and thinning out behind of the emitted radiation here referred to is obviously the shortening in front and lengthening behind of the emitted waves, constituting the Doppler-displacement, and on the authority of the theory of relativity we are told that the possibility of such shortening and lengthening is a suggestion which "may . . . be at once dismissed as fallacious." The change of frequency in front due to this denied shortening is $c/c - v$ and of the denied lengthening behind is $c/c + v$. And it is worth

[14] Dingle, H., "Modern Astrophysics," pp. 35, 36.

[15] Eddington, A. S., "The Nature of the Physical World," pp. 58, 59.

noting for future reference that the Einsteinian unit-changer $\sqrt{c^2/c^2 - v^2}$ is simply the geometric mean of these denied changes of frequency.

Eddington's statement to be sure refers to emitted radiation only, but according to the relativists, it holds equally true of received or observed radiation. Thus Tolman says:

"A motion of the source of light towards the observer is identical with a motion of the observer towards the source." [16]

This conclusion in fact follows from the principle of the constancy of velocity of light, a principle which would be as clearly violated by change in the relative velocity of light and observer (or light and spectroscope) as by change in the relative velocity of light and source. Thus, according to the relativists, radiation can no more heap up or thin out relative to its sink than relative to its source.

Parenthetically, and as a digression, however, it should at this point be explained that these denials of the relativists are only apparent; and this is true of the denials of physical causes involved in dimensional explanations generally. The reason for this is as follows: When units are redefined the meanings of words which involve those units are redefined also. Hence Einstein's redefinitions of time and length have provided physics, not only with two meanings of the words time and length, but two meanings of the words area and space and velocity and energy, etc. Redefinition of the fundamental units involves redefinition of the derived units, and this is the origin of the confusion and paradox which beset the theory of relativity. The relativists and non-relativists are not really contradicting each other, any more than Eddington is contradicting the classical physicists when he says the Doppler effect is impossible and they say it is possible. The disagreements are delusions. The contradictions are only apparent. They are ambiguities posing as contradictions; for the relativists do not mean what they appear to mean. If space permitted endless examples of these ambiguities simulating contradictions and confounding the relativists themselves might be cited, as in the above instance, where Eddington apparently denies the existence of

[16] Tolman, R. C., "The Theory of the Relativity of Motion," p. 22.

a phenomenon familiar to every astronomer, and which he himself has often observed in the spectroscope. This apparent self-contradiction arises because he confounds two different meanings of the word "velocity." Measured in the Einsteinian variable units the relative velocity of matter and light cannot vary, because in this case the unit of velocity has, in the words of Eddington, "made itself a constant fraction" of the velocity to be measured. Measured in constant Newtonian units it can and does vary.

A parallel though simpler example of ambiguity of this type may make this puzzling matter clearer. It will be generally admitted that the motor cars, to be seen on our roads, travel at different, that is, *non*-constant "velocities." Very well: call the velocity of any car at any moment v. Then to prove that these variable velocities are nevertheless constant and equal to the velocity of light, we have only to postulate the expression c/v as our non-Newtonian unit-changer and the deed is done, because $v \times c/v = c$. Measured in the specified variable non-Newtonian units, v cannot vary; measured in constant Newtonian units it can and does. Thus the Newtonian who claims the velocity of motor cars *can* vary and the non-Newtonian who claims it *cannot*, are not contradicting each other. They are in no disagreement. It is the ambiguity caused by the variable unit which leads them to suppose they are.

The dispute between the relativists and their opponents indeed, is exactly parallel to the dispute between opponents who disagree about the color of John's hair when, unrealized by the disputants, there are *two Johns*, one a black-haired boy and the other a red-haired one, the former being the John referred to by one of the disputants and the latter the John referred to by the other. The mystery of how John's hair can be black and yet not black is dispelled when we realize that there are two "Johns." Similarly the mystery of how the relative velocity of light and variably moving bodies can be constant and yet not constant is dispelled when we realize that there are two "velocities." The paradoxes involved in these ambiguities disappear when we ask "which John?" or "which velocity?" A few examples will illustrate how the original redefinitions of Einstein cause ambiguities

which pervade the language and lead to endless misunderstandings, causing entirely true and commonplace statements to take on aspects most nonsensical and bizarre.

Consider for example Eddington's statement that "So long as he [a youthful adventurer] travels with the speed of light he has immortality and eternal youth." [17] This sounds nonsensical, yet it is a correct statement and the relativists can easily prove it. For to say that a person's youth will be eternal is to say he will live a long *time*. But *which* time? He can live millions of years of the black-haired Newtonian time and yet live exactly zero years of the red-haired Einsteinian time, so that as the centuries roll, time for him will stand still. In short, he will never grow old in the Einsteinian sense of "old." Or take the following claim of Lewis: "I claim that my eye touches a star as truly as my finger touches this table." [18] This also sounds nonsensical, but the relativists have no difficulty in demonstrating that it expresses a mere truism like "four quarts make a gallon." For to say that two objects "touch" or are in contact with each other is to say that the distance between them is zero—and distance is measured in units of *length*. But *which* length is being referred to? Two objects can be separated by billions of miles of the black-haired Newtonian length and yet be separated by exactly zero miles of the red-haired Einsteinian length. Thus Einstein's redefinition of "length" has changed the meaning of a word so seemingly remote from the relativity theory as the word "touch." There is really no paradox in the statement of Lewis. There is only an ambiguity simulating a paradox. The contradictions of this, as of the other strange statements made by the relativists, disappear when we realize that there are *two Johns*—and their hair is not of the same color. In the examples just cited for instance, John has changed his name, first to Time and then to Length.

To make matters plainer recall that both time and length intervals are by the relativists measured in units which are multiplied by the unit-changer $1/\sqrt{1 - v^2/c^2}$. Now the numerical magnitude of any interval obviously depends upon the unit in which it is measured. Expressed in feet a yard-

[17] Eddington, A. S., "Space, Time and Gravitation," p. 26.

[18] Lewis, G. N., "The Anatomy of Science," p. 133.

stick has the numerical magnitude 3; expressed in inches its magnitude is 36. The larger the unit in which a thing is measured the smaller the numerical magnitude and *vice versa*. But notice that the Einsteinian unit-changer is larger for high values of v than for low ones. This is the reason why Einsteinian time and length (in the direction of motion) get progressively smaller the higher the velocity. It is simply because the units in which they are measured get progressively larger. And would it be reasonable to deny that, measured in such units, they could do anything else? This also explains the insistence by Eddington and Lewis that both time and length intervals become zero when $v = c$. You have only to substitute the value c for v in the unit-changer to see that what they say is true. It is true because the unit in which the intervals are measured becomes infinity. A youthful adventurer therefore who moves with the velocity of light cannot grow old because, no matter how many Newtonian centuries he lives, the non-Newtonian interval represented by his life must always be zero, or no time at all; and similarly the length of a beam of light extending from a star to Lewis's eye must always be zero because, as light moves with the velocity c, the length of any beam in the direction of its motion must be measured in infinitely large units. Just as all light beams have a constant velocity of c relative to all frames of reference irrespective of the relative velocities of such frames, so all such beams have a constant length of o between all frames of reference irrespective of their distance apart. And these laws of light are, as Russell reminds us (page 345), truisms which "result from our methods of measurement," just as all other relativistic laws of nature are. In a similar manner it may readily be shown that the other strange sounding statements of the relativists are entirely correct. To this kind of ambiguity for instance may be traced the astounding (!) discovery by certain relativists that the relation of cause and effect can be reversed in time. As Weyl says: "In principle [according to the theory of relativity] it is possible to experience events now that will in part be an effect of my future resolves and actions." [19] That events in

[19] Weyl, H., "Space—Time—Matter," p. 274.

the future can causally influence events in the present is a statement which follows, not from any new discovery about the law of causation, but simply from new meanings of the words "present" (or "now") and "future" consequent upon the changed meaning of the word "time." The origin of the idea, however, is, on our hypothesis, quite manifest. When a wave-emitting body moves faster than the waves which it emits, then to all observers toward whom it has been moving, the Doppler effect will reverse itself, and hence the waves will be observed in the reverse order from that in which they are emitted. The later a wave is emitted in time, the earlier in time it will be observed. The reason for this is obvious. But for observers away from whom the body has been moving there will be no reversal of the Doppler effect at all. Now according to the theory of relativity, events at a moving point body occur in the order in which they are observed by means of light signals. Hence if a wave A arrives from such a body before a wave B, it means that willy nilly the emission of A occurred before that of B. And as the theory bans physical mechanisms, it cannot take into account the peculiar Doppler mechanism which, at velocities greater than c, can cause later emitted waves to become earlier observed ones. Hence this reversal is mistaken for a reversal of time itself.

That this is the origin of the error is sufficiently indicated by the fact that the only place in which the relativity reversal of time occurs is on a body observed as *approaching* at a velocity greater than c. On one *receding* at such a velocity no reversal occurs. Indeed if the relativity equations did not lead to this result they could not agree with the facts of the Doppler effect. Thus it is not time which reverses itself on certain coördinate systems when v is greater than c. It is simply the physical mechanism which the relativity equations are disguising. It is not the law of causation which is running backward. It is only a Doppler effect.

Most relativists avoid the dilemma of time's reversal by claiming that v can never be greater than c. But to some, such as Weyl, the claim is evidently deemed unsound, and thus the reversal of the physical cause back of the Einstein equations leads them to assume that the law of causation

itself can operate backward. Such misunderstandings inevitably arise from the attempt to ignore mechanisms and models and accept as a guide "some mathematical expression . . . which cannot be further analysed." This process, though recommended by Eddington and various other modern physicists, is too blind to be safe.

Having identified the physical agency which is denied (i.e. apparently denied) by Einstein's unit-changer, let us now return to the six confirmations of the theory derived from that unit-changer enumerated on page 5, to see if they will verify the identification. And let us direct attention at the outset to the first four confirmations, namely: (1) The Doppler effect in light, (2) The aberration of light, (3) The Fizeau effect, (4) The Airy effect.

These four phenomena are predicted by the special theory of relativity. Are they all Doppler or aberrational effects? That the two first named are such effects will be questioned by no one. That the two last named are also effects of the same character will be questioned by no one familiar with the experiments which detected the effects. For the Fizeau effect is simply the light displacement caused by the movement of the medium through which the light is passing, instead of by the movement of the source or the observer—a tertiary Doppler effect. (Water was the medium used by Fizeau.) And the Airy effect bears the same relation to aberration that the Fizeau effect bears to the ordinary Doppler effect. Airy filled his telescope with water and found the angle of stellar aberration unchanged, because of the displacement of the star-light by the water moving with the earth, an effect which would have been impossible in the absence of change in the relative (Newtonian) velocity of water and light.[20]

The first four confirmations of the special theory of relativity then are undoubted Doppler (or aberrational) displacements, and thus our interpretation of the physical cause which that theory is dimensionally disguising is verified so far as the first four confirmations are concerned. In these four cases the duplication is obvious and denied by no responsible physicist. The relativity explanation is not a

[20] See Preston, Thomas, "The Theory of Light," 5th edition, pp. 561, 563.

separate explanation. It is the same one expressed in novel units of measurement. It apparently denies the possibility of Doppler-displacements yet predicts them as well as the classical theory. It does not apply the correction for motion twice, as it would have to do if the so-called "relativity of time and space" were a separate, independent means of explanation different from the changed velocity between matter and light. The correction is applied only once and thus the result coincides with the classical predictions and the facts. Einstein's assertion, to be found in Section 13 of "Relativity," that his explanation "in no way opposes" the classical explanation of the Fizeau effect, may in fact be extended to the other three confirmations. In all four cases the relativity explanation "in no way opposes" the non-relativity explanation, for the very good reason that it is the same explanation transferred to units of measurement. These first four confirmations therefore provide veritable Rosetta Stones of relativity, because the dimensional and non-dimensional explanations appear side by side, the changed units of time and length in one column yielding the truistic dimensional expression of the phenomena, and the Doppler effect in light in the parallel column yielding the physical cause. And any physicist can see for himself that there is no opposition between them. The explanations are the same. It is only the language that is different.

This brings us to the consideration of the two confirmations of the relativity theory last named on page 348, and these merit our particular attention since, in contrast with the first four, the classical theory seems to provide no duplicate physical explanation except the electrodynamic explanation of Lorentz, to which many physicists, including Einstein, have objected because of its *ad hoc* character. Does the interpretation of the relativity unit-changers as disguises for Doppler effects provide one which is free from this objection? Let us see.

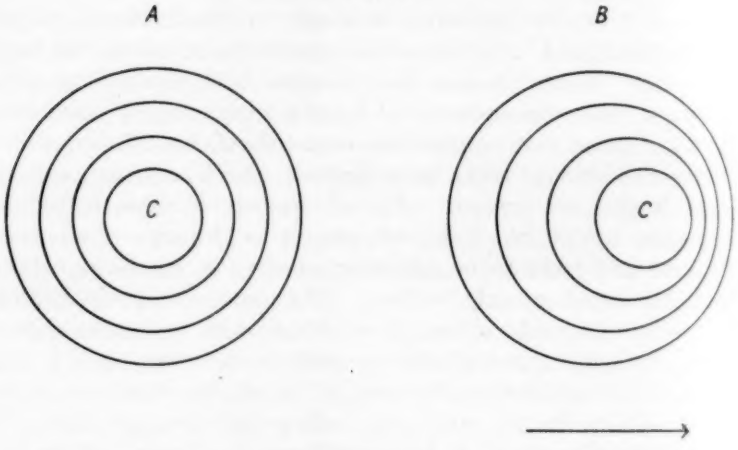
Consider to begin with confirmation number 5. This is afforded by the Kaufmann experiment on the increase of inertia, i.e. inertial mass, with motion, and shows that the inertia of a body increases in the ratio $1 : 1/\sqrt{1 - v^2/c^2}$ when the velocity of the body is expressed by v and the velocity of

light by c. This increase of inertia with velocity is predicted by the Einstein equations, and hence, by Sommerfeld, the Kaufmann experiment is called "the *experimentum crucis* of the theory of relativity." [21] Can such an increase of inertial mass with velocity be the expression of a Doppler displacement, as our interpretation of the Einstein dimensional explanation requires? The answer depends upon what assumption is made about the constitution of matter. Modern physicists, relativists as well as non-relativists, tell us that mass, and hence matter, is composed entirely of energy. Einstein in particular insists that this is the case. Now energy can be divided into two kinds: (1) Radiational energy or radiation, and (2) Non-radiational forms of energy or non-radiation. If we assume that matter is composed of non-radiation, then the increase of inertia with velocity shown by the Kaufmann experiment does not follow, but if we assume it is of radiational form, new light on the Kaufmann experiment begins to appear. For if matter is constituted of radiation, it will, like light, be subject to Doppler effects, and this fact will have to be taken account of in reasoning about the behavior of moving bodies. It seems therefore justifiable at this point to take a hint from the modern quantum theory which recognizes a material particle as a wave-packet, and assume that matter is composed of mechanical wave-quanta, of a structure for the most part unknown, but approximating perhaps to the state of things shown in Fig. 1, where the radiation is represented as emitted from centres or nuclei, such as C, in a manner analogous to the emission of light from a star. From evidence that need not be cited here we may provisionally regard the radiation thus emitted to consist of super-frequency "particles" moving, like photons, with the velocity of light. We shall not here and now inquire where the energy of these wave-packets comes from or goes —that would take us too far afield—but shall point out that if the inertial mass of bodies consists of quanta having this structure, or one subject to like Doppler effects, consequences follow which physicists cannot reasonably ignore. For Larmor has shown [22] that under these circumstances an

[21] Sommerfeld, A., "Atomic Structure and Spectral Lines," p. 464.
[22] Larmor, J., "Mathematical and Physical Papers," Vol. 2, p. 447.

increase of inertia must occur in the ratio $1 : 1/\sqrt{1 - v^2/c^2}$, which is the ratio of increase found experimentally by Kaufmann. Moreover, Larmor's calculation proves that this is a Doppler effect, due to the heaping up of radiation in front of the radiating centre and its thinning out behind, a change which results in an increase of energy, because the shortening of wave-length in front approaches zero as the velocity of the centre approaches that of light, whereas the lengthening of wave-length behind approaches twice the stationary value.

FIG. 1.



As the energy-density of radiation is inversely proportional to the square of the wave-length, it is obvious that increase of velocity, involving as it does a Doppler-displacement, must, within a constant volume, mean increase of energy and Larmor's law is simply the mathematical expression of the dependence of this increase on velocity. The law may be understood by again referring to Fig. 1, where *A* represents the stationary, and *B* the moving radiant particle. The series of circles are cross-sections of successive wave-fronts. The arrow at *B* indicates the direction of motion of the centre, and the non-concentric circles indicate the shortening of the waves in front and their lengthening behind the moving radiating centre *C*. If then we assume that material particles are composed of radiation having the structure indicated, or

any structure subject to a like Doppler-displacement, the fifth confirmation of the relativity theory falls naturally into the place assigned to it by our interpretation. It, like the first four, is the expression of a Doppler-displacement. And it may be recalled that we have arrived at this result simply by adopting the accepted view of the quantum theory that matter is composed of waves, only we have provisionally assumed something about the manner in which the waves travel which is not assumed by that theory.

But this is by no means the end of the matter. For, from the same assumption regarding the wave-structure of matter which gives us the Kaufmann increase of mass with motion, we are enabled, entirely independent of the theory of relativity, to obtain the relativity formula for the kinetic energy of a moving body and of the change thereof. For if ΔE signifies the change in the kinetic energy of a body due to any change in its velocity, and Δm its change in mass due to the same change in velocity, then, by means of mathematical processes of exclusively classical character, it can be shown that $\Delta E = c^2 \Delta m$.[23] And this is the exact change of energy which the changed Doppler effect due to said change in velocity calls for on the assumption that matter possesses the wave-structure illustrated in Fig. 1.

Furthermore, the relativity expression for the energy of which a stationary mass is composed is mc^2 ergs where m is the stationary mass expressed in grams, and this agrees with our assumption that matter is composed of super-frequency photons moving, like other photons, with velocity c, the kinetic energy of which, like that of other "particles," is proportional to the square of their velocity. It also agrees with the quantum theory which identifies particles with waves, the energy of which is kinetic.

Thus from our hypothesis that matter is composed of radiation, subject like other radiation to Doppler effects, we are able to arrive at the relativity results concerning the relation of matter to energy without any change in the definitions (or concepts) of time and space—which constitutes further evidence that the relativity redefinitions are disguises

[23] See Tolman, R. C., "The Theory of the Relativity of Motion," p. 40.

for Doppler effects, whether those effects are in light waves or mechanical ones.

But if matter is composed of radiation, then not only should its mass and kinetic energy thus increase in a manner calculable from the law governing Doppler effects, but it should, like other forms of radiation, be subject to interference effects of diffraction. Have any such effects been detected? It is a fact well known to physicists that they have. Davisson and Germer diffracted electrons in 1927, and from their experiments Davisson came to the following conclusion:

"During the last few years we have come to recognize that there are circumstances in which it is convenient, if not indeed necessary, to regard electrons as waves rather than as particles, and we are making more and more frequent use of such terms as diffraction, reflection, refraction and dispersion in describing their behavior. . . .

"The evidence that electrons are waves is similar to the evidence that light and X-rays are waves." [24]

Can a thing which is composed of waves and is subject to "diffraction, reflection, refraction and dispersion" be a thing which is *not* subject to Doppler effects? To so assume is to assume something entirely unknown in nature, and the physical possibility of which has never been proved. The burden of proof therefore is on him who *denies* that matter is subject to Doppler effects.

This experimental detection of the wave-structure of matter, which has been confirmed by the work of many experimenters, stands on its own feet, independent of any theory. The diffraction of atoms, as well as of electrons, has been experimentally attained. Although there is much agreement between the prevailing wave-mechanics theory and experiment, there is also disagreement, and in some respects the theory is unsatisfactory. Its assumption of a phase-velocity varying from c to infinity, for instance, is unnatural from a physical point of view, and is moreover in conflict with the theory of relativity which denies the possibility of velocities greater than c.[25] There is in fact much doubt about the

[24] Davisson, C. J., *The Bell System Technical Journal*, Vol. 8, p. 217.

[25] The lack of agreement among relativists about what c is the limiting velocity *of*, illustrates the uncertainty with which they interpret their own symbols.

physical reality of the waves postulated by the theory. Some regard them as no more than convenient mathematical fictions, and Newman in summing up the evidence concludes that—

"The point of view which appears at present most satisfactory is one that supposes the waves to have no physical significance." [26]

That matter has a wave-structure which is physically real, however, is now generally conceded by physicists, for experiment, independent of any theory, clearly attests it. The question which remains relates to the character of the waves, and this question is still open. No attempt is here made to advance more than a provisional theory of their character, but in view of the agreement between Larmor's calculated, and Kaufmann's observed, law of the increase of inertia with velocity, it seems not unreasonable to point out that matter behaves "as if" its wave-structure were that illustrated in Fig. 1, or a structure subject to like Doppler-effects. That the assumptions of deBroglie, on which the wave-mechanics theory has been erected, agree with the particular conclusion here drawn from Larmor's law, that inertial mass will increase with velocity according to the law discovered by Kaufmann,

---

Thus Einstein says it is the limiting velocity of a "real body" ("Relativity," p. 36); Tolman says of a "causal impulse" ("Theory of the Relativity of Motion," p. 55); and Eddington says of a "signal" ("Nature of the Physical World," p. 57); while Campbell ("Modern Electrical Theory, Chapter xvi, Relativity," p. 21) and Silberstein ("The Theory of Relativity," p. 114) deny that it is a limiting velocity at all. The idleness of trying to decide what c is a limiting velocity *of* is made evident by Carmichael, who says: "If a material body had a velocity greater than that of light, the numerical measure of length and time [and hence of their ratio, velocity] on that body would be imaginary." ("The Theory of Relativity," p. 60.) A glance at the unit-changer on page 350 shows the correctness of this statement. For when v is greater than c the unit of time, as well as of length, contains the square root of a negative number, which is as contradictory as an odd number that is even. Thus a non-Newtonian velocity greater than *c of anything* is as much a contradiction as a square circle or a Newtonian velocity greater than infinity, and hence the same kind of an impossibility as these. In this connection it is of interest to note that x-rays in material media have fractional indices of refraction, which means that their Newtonian velocity is greater than c, and this in turn means that to speak of their non-Newtonian velocity is to speak of a contradiction.

[26] Newman, F. H., "Recent Advances in Physics," p. 19.

is made plain in his original thesis, and will be found mathematically expressed in a more recent volume, his comment thereon being as follows:

"In particular it is easy to show that this point of view permits of a complete explanation of the various Doppler effects." [27]

Whether the assumptions here made are reconcilable with all the verified results of deBroglie's theory, as they seem to be with this one, is a matter which has not been put to the test, but until they have been shown in conflict with some fact of experience, they appear entitled to the status of verified assumptions. The reservation that a wave structure subject to Doppler effects of similar magnitude to those illustrated in Fig. 1, would be equivalent thereto, so far as these assumptions are concerned, should, however, be emphasized. The figure is diagrammatic, not photographic. The waves of which material particles are composed may, and in all probability do, rotate about a nucleus of some sort in the manner assumed by deBroglie, but such a structure would be subject to Doppler effects, and in view of deBroglie's statement, they would presumably be of similar magnitude. At least, on any other interpretation, his theory can be reconciled neither with the theory of relativity nor Kaufmann's experiment.

So much for the fifth confirmation of the relativity theory. Now let us return to the sixth and last provided by the null result of the Michelson-Morley experiment—the best known confirmation of the relativity theory. Can this be due to a Doppler-displacement also? Well, consider the inference which by relativists and non-relativists alike, is generally conceded to follow from the result of that experiment. It is that the diameter of a material particle is caused to contract in the direction of its motion in the ratio $1 : \sqrt{1 - v^2/c^2}$, but in directions at right angles thereto it is not caused to contract at all—a type of asymmetry certainly suggestive of a Doppler-displacement when we recall the fact that the displacement caused by a source velocity v produces in the direction of motion a double change of wave-length the geometric mean of which is $\sqrt{1 - v^2/c^2}$, but in directions at right angles thereto

[27] deBroglie, L., and Brillouin, L., "Selected Papers on Wave Mechanics," p. 13.

produces no change at all. This is called the Fitzgerald or Lorentz contraction and is evidently a phenomenon of impenetrability. Longitudinally the mutual penetrability of the units of which bodies are composed increases whereas transversely it does not. The atoms in the cubic space-lattice of a moving salt crystal, for instance, would, in the direction of motion, be nearer together than in directions at right angles thereto, and this increased longitudinal penetrability would at high velocities approach one hundred per cent. Moreover, it is a case of variation of impenetrability of so uncomplicated a character that it would seem to be particularly well adapted to throw light on that very mysterious property of matter. The variation seems to be due to variation in absolute motion alone (motion relative to the ether). I am aware that the relativists appear to deny that the motion is absolute, as they appear to deny the Doppler and aberrational effects, but in reality they deny the one no more than the other. Their apparent contradiction of the non-relativist on this, as on other, issues is an ambiguity posing as a contradiction. When Eddington in the passage recently quoted says "There is no absolute velocity . . . for the star to come to" he is denying absolute non-Newtonian velocity, but this, far from being a denial of absolute Newtonian velocity, is an affirmation of it, for if it were not, his (apparent) denial of the Doppler effect would be confirmed by observation. The two "Johns" indeed are so confusing that the relativists hardly know what to say about the reality of the Lorentz contraction. Eddington, for instance, perceives that there is an ambiguity of some kind involved in this question, but instead of tracing it to the original redefinitions of "time" and "length" and hence of their ratio "motion," he attributes it to an ambiguity in the meaning of "true"! Here is what he says about it:

"Is it really true that a moving rod becomes shortened in the direction of its motion? It is not altogether easy to give a plain answer. I think we often draw a distinction between what is *true* and what is *really true*. A statement which does not profess to deal with anything except appearances may be *true;* a statement which is not only true but deals with the realities beneath the appearances is *really true*. . . . The shortening of the moving rod is *true*, but it is not *really true*.

It is not a statement about reality (the absolute) but it is a true statement about appearances in our frame of reference." [28]

You will recall that some 350 years ago Galileo dropped simultaneously two shot of unequal weight from the leaning tower of Pisa which reached the ground at the same time, thereby refuting the ancient dictum of Aristotle that the heavier a body is the faster it will fall. You will also recall perhaps that his colleagues who witnessed the experiment are said to have concluded that, notwithstanding Galileo's result, Aristotle was *really* right in his contention. I have sometimes wondered how they could have come to such a strange conclusion; but Eddington's distinction between the *true* and the *really true* makes their mental process plain. They evidently made the same distinction. All Galileo showed them was an appearance. The two shot *appeared* to strike the ground at the same time; but they, like Eddington, were interested, not in the appearance, but in the reality beneath it. Galileo's contention therefore was *true*, but it was not *really true*, and so Aristotle was right after all.

It is evident that what puzzles Eddington and leads him to the self-destroying conclusion that there are truths which are not really true, is the peculiar "now you see it and now you don't" character of relativity effects. The shortening of the moving rod (or earth) is observable by one relative to whom the body is moving, but not to one on the moving body itself. And as observability is the best test of reality, it is real (or *really true*) to the one, but not to the other. And this is certainly an exceedingly puzzling phenomenon if we fail to recognize that the shortening is a Doppler effect. Once this is recognized, however, the puzzle disappears, for all Doppler (and aberrational) effects have this character. Even sound waves have it. When two speeding motor-cars with horns sounding pass each other, the drivers of each will observe a sudden fall of pitch in the sound of the other's horn as the cars pass, due to the change from the heaped up waves in front as the cars approach to the thinned out waves behind as they recede. But neither driver will observe any change in pitch in the sound of his own horn, because his

[28] Eddington, A. S., "The Nature of the Physical World," pp. 33, 34.

own movement cancels all Doppler effects which arise from that movement. Any two persons equipped with motor-cars can easily try this experiment, and thus satisfy themselves of this peculiar "relativity" effect which "is *true* but . . . is not *really true*." Indeed, by substituting the velocity of sound for the velocity of light c in the relativity formulæ, the Doppler effects in sound can all be predicted. The increase of mass and Lorentz shortening, however, are not to be found among the Doppler effects in sound, and the reason for this omission is quite plain. It is because these two phenomena are Doppler effects in the waves of which matter is composed, but matter is not composed of sound waves.

The nullifying influence of motion on the Doppler displacements which it causes results in what the relativists call the restricted principle of relativity, expressed by Robertson thus:

"It is impossible to detect a uniform motion of the earth or any system through space by observations made on that system." [29]

And by Eddington as follows:

"*It is impossible by any conceivable experiment to detect uniform motion through the ether.*" [30]

It is plain that this principle, so much stressed by the relativists, is readily deducible from our hypothesis that relativity effects are Doppler effects, and hence tends to confirm it.

Now the evidence indicates that the Lorentz contraction is not only true but really true; at any rate true in the sense that the Doppler effect (which also varies with the frame of reference) is true—an effect which Eddington also says (see page 353) is not really true, despite the fact that he has personally observed at least its "appearance" in the spectroscope. And I shall here attempt a hypothetical physical explanation of it, which I will call the radiation hypothesis.

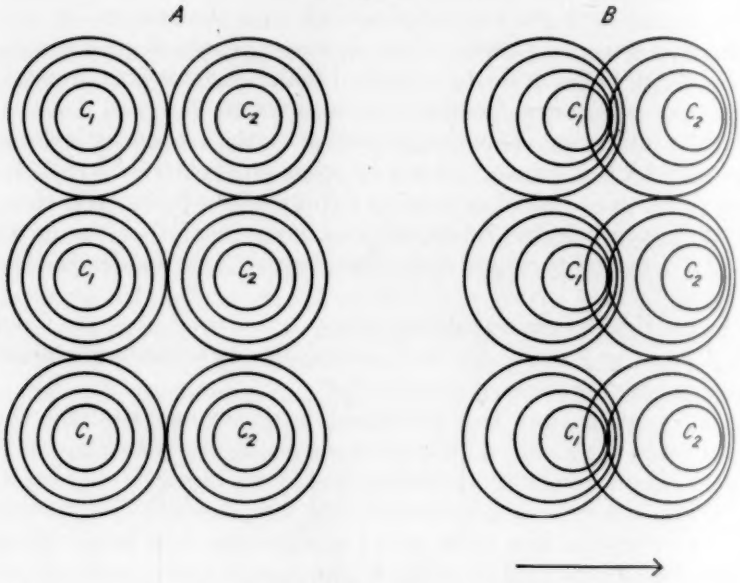
This may best be illustrated by the help of Fig. 2, in which A again represents the stationary particles and B the moving ones, the arrow indicating the direction of motion in B, the rate of which (velocity) is v. Six particles are illustrated by the circles, concentric in A, non-concentric in B. The longitudinal (horizontal) distance between the centres

[29] Robertson, J. K., "Introduction to Physical Optics," p. 402.

[30] Eddington, A. S., "Report on the Relativity Theory of Gravitation," p. 5.

C_1C_2 (the distance in the direction of motion), in B may be called D_v, the corresponding distance in A, which is the same in all directions, being called D_0. The contraction assumption requires that stationary particles in equilibrium, as in A, assume a new position of equilibrium when the body of which the particles are components, is set in motion in the direction

FIG. 2.



of the arrow in B with velocity v, and a new position of equilibrium established, D_0 becoming D_v, a shorter distance. If *any* wave-mechanics theory is correct, either the theory here proposed or that of deBroglie, *all* reactions between material bodies are reactions between waves, because said bodies are composed of waves and nothing else. On our assumption then, the readjustment of equilibrium between the particles must be due to a reaction between waves. We shall further assume that the position of equilibrium between C_1 and C_2 in all cases is determined by an equality between the forces of attraction urging the centres together, and that of repulsion urging, or keeping, them apart; and shall assume that the force of attraction is the inverse square law generally

postulated in such cases. The usual assumption that the attraction is proportional to the product of the masses or charges may also be adopted without change. So far I think the assumptions are in accord with past experience and will be generally accepted. Now what assumptions shall be made about the repulsive forces between the centres? This raises two questions: First as to the intensity of the force at a given distance, and second as to its variation with distance. As intensity (in the case of attraction) is proportional to mass and hence to energy, and as the energy of a quantum is proportional to its frequency, we may plausibly assume that the intensity of repulsion is proportional to frequency, which means that the repulsion between two particles is proportional to the product of the frequencies of the waves which, in the process of radiation, impinge upon each other. As the frequency of all quanta is proportional to their mass, it is plain that by this assumption we bring the impenetrability of matter into close accordance with the other two fundamental properties of matter—inertia and gravitation—which are also proportional to frequency because proportional to mass. Indeed, according to our hypothesis, it is because inertia is proportional to frequency that it varies with velocity. These frequencies are those relative to the ether. The frequencies relative to the centres C_1 and C_2 remain constant owing to the compensation effect of their common motion.

Now the variation of the repulsive force of impenetrability with distance is presumably not an inverse square law, for impenetrability, in contrast with gravitational, electric and magnetic forces, is a reaction confined to close quarters. There is no repulsion between two uncharged steel balls until their surfaces actually touch. By the same reasoning we may surmise that it does not vary with distance according to the first or the third or any other power. I am aware that the usual assumption about the mutual forces between particles at close quarters is that they vary inversely and rapidly with the distance, but the objections to this are two in number. In the first place it involves the assumption that the forces, both of attraction and repulsion, become infinite at the centre of the particle, which as a physical assumption is obviously objectionable. And in the second place this

assumption has led to unsatisfactory results. Thus Jones [31] found that the assumption of forces varying with powers of the distance, when extended to close quarters, required variations of the attracting force from an inverse fourth to an inverse sixth power, and of the repulsive force from an inverse ninth to an inverse fifteenth power; and sometimes fractional powers were found to fit the facts better than integral ones. These curious results are evidently due to the complexity of the causes at work. The conditions of mutual contact of molecules in gases and crystals on which his results rested were, even under the most favorable conditions, so complicated that the chance of discovering the real character of the forces at work was not promising. The Lorentz contraction, on the other hand, seems to present an example of variation of material penetrability (with variation of absolute motion alone as the cause) so uncomplicated, that none is likely to be found which offers a better opportunity of detecting the nature of the forces acting. Without attaching undue importance to what can only be a guess therefore, I shall make the simplest assumption possible about the repulsive force of impenetrability, namely, that it does not vary with the distance at all, but is confined to some reaction between waves in the initial stages of the process of emission before they get fairly started on their expanding way, a process the details of which are unknown. When once fairly started ordinary waves do not impede one another—they are mutually completely penetrable. We may therefore conclude that their impenetrability is a phenomenon of extreme close quarters, connected in some manner with the process of emission itself before the property of mutual penetrability is developed. I shall therefore assume that, within the very small limits where this type of repulsion occurs at all, the repulsive force remains constant. This I think is the only *ad hoc* assumption I have made, the others following the precedent of experience, and in approaching a problem as difficult as the cause of impenetrability one *ad hoc* assumption is perhaps allowable. At any rate, I can cite for justification the two *ad hoc* definitions which Einstein formulated to

[31] Jones, J. E. L., *Proc. Roy. Soc.*, Vol. A-106, pp. 441, 463, 709, and Vol. A-112, pp. 214, 230.

explain dimensionally the same result of the same experiment. Thus the non-dimensional explanation requires only half the *ad hoc* assumptions required by the dimensional one.

Expressing the foregoing assumptions mathematically, we have the following:

Let the force of attraction between C_1 and C_2 in the stationary condition be our unit of attraction, and the frequency of the radiated waves in that condition be our unit of frequency. Let A_v be the attractive force, and R_v the repulsive force at distance D_v, which is the distance where equilibrium between the forces occurs when the velocity is v. Then $A_v = D_0^2/D_v^2$. The velocity of the waves is c. Hence at velocity v, the frequency of the shortened impinging waves from C_1 (see B, Fig. 2) will, on account of the Doppler effect, be $c/c - v$ and of the lengthened impinging waves from C_2 will be $c/c + v$, and thus on our assumption, $R_v = c^2/c^2 - v^2$. But at the point of equilibrium $A_v = R_v$. Hence $D_v^2/D_0^2 = c^2 - v^2/c^2 = 1 - v^2/c^2$ or $D_v = D_0\sqrt{1 - v^2/c^2}$, which is the formula for the Lorentz contraction. At right angles to the direction of motion, however, there is no Doppler effect; hence no change in the frequency of the impinging waves, and hence no contraction, a consequence of our assumptions which checks with experiment. As all the particles of which a body is composed would be subject to this contraction in the degree to which they were subject to the Doppler effect, the body would change its configuration accordingly. A sphere for instance would assume the form of an ellipsoid of revolution with its long axis perpendicular to the direction of motion.

Of course this agreement with experiment may be a mere coincidence, and I am not advancing it as a confirmation of the radiation hypothesis as satisfactory as that afforded by the Kaufmann experiment, but taken in conjunction the two confirmations mutually reinforce each other. The last named confirmation is as strong as that for the theory of relativity. The simple assumption that a material particle is composed of waves radiating from a centre with velocity c is all that is required. If the Kaufmann experiment is the *experimentum crucis* of the theory of relativity, as Sommerfeld maintains, it is certainly an *experimentum crucis* of the radiation hy-

pothesis also. The non-dimensional explanation is confirmed no less crucially than the dimensional one. The result in the case of the Michelson-Morley experiment is, on account of the further assumptions called for, admittedly weaker. Nevertheless, there is some reinforcing evidence that it is correct.

In the first place it results from very simple assumptions, only one of which is *ad hoc*. In the second place the hypothesis fits the requirement that any explanation of the contraction shall appeal to the same cause as the explanation of the increase of mass, since these two effects are concomitant, one being the reciprocal of the other—thus calling for a common cause. In the third place it harmonizes with results of the many experiments which show that matter is composed of waves, indicating that the facts of impenetrability can find their explanation only in reactions between waves. And in the fourth place it appeals to the same physical agency which the relativity theory has been found to express dimensionally in the five other confirmations already enumerated, namely, a Doppler-displacement. This contraction, therefore, like the increase of mass, would be detectable only by an observer moving relative to the body subject to it. It would, like any other Doppler effect, be undetectable by an observer on the body itself. Hence the negative result of the Michelson-Morley experiment.

Thus if the radiation hypothesis is correct, the last two confirmations of the relativity theory are, like the first four, Doppler-displacements in radiation moving with velocity c. And if that hypothesis is not correct, it can at least be said that material bodies behave just as they would behave if it were correct. Their inertial mass increases and their configuration changes in the manner called for by the hypothesis, quantitatively as well as qualitatively. Moreover, if the hypothesis be denied, we are confronted with the problem of explaining how a dimensional explanation which unquestionably disguises a Doppler-displacement in the first four cases of confirmation, disguises something entirely different—yet a function of velocity alone—in the last two. But whether correct or incorrect, the hypothesis in any event makes all six confirmations hang together as effects of a common

physical agency dependent for its manifestation upon relative motion, and thus explains, not only the unification achieved by the relativity theory, but also its success in predicting the experimental results which baffled the classical physicists. This explanation is as follows:

At the time of the promulgation of Einstein's special theory it was becoming evident to physicists that matter was composed of energy of some kind, and to-day this position is taken by physicists in general, relativists as well as non-relativists. But the *kind* of energy of which it was composed was not specifically postulated. The tacit assumption, however, was that it was of non-radiational form, since had the explicit assumption been made that it was radiational the suspicion would naturally have arisen that it would, like radiational energy in general, be subject to Doppler effects, and in allowing for them, a duplication of the relativity results would have been disclosed. Einstein in approaching the problem made assumptions which coincided with the postulates of the classical physicists. He assumed matter to consist of energy but tacitly assumed it was of non-radiational form. At least no relativist ever suggests that matter is composed of anything subject to Doppler effects. This assumption being contrary to fact, as we here maintain, required for its correction some substitute or disguise for the Doppler effects to which moving bodies are actually subject. And this disguise was provided by Einstein's new units for time and length comprising his dimensional explanation by means of the so-called "relativity" of those dimensions. By failing to assume matter to consist of radiation he failed to make allowance for Doppler effects in moving matter in the normal classical manner, but he atoned for this failure by expressing said effects in his new units in the manner already explained, and thus, while dispensing with the physical cause, successfully predicted its effects without specifying any cause, by this means making the allowance in the normal dimensional manner. Having accomplished this for the Michelson-Morley and Kaufmann experiments, and thus achieved a unification beyond what classical mechanics could at that time achieve, he discovered that his theory would also predict the four other results of

experiment already discussed—and very naturally so, since these are all Doppler effects also. Additional confirmation was thus given his equations, and solidified belief in the "discovery" of the truism that (relative) time and length are relative. And of course if length is relative, space, whose dimensions are length cubed, must be relative also. What the classical physicists overlooked, however, was not an unsuspected property of time and space, as so commonly supposed. It was an unsuspected property of matter. They did not suspect it to be composed of anything subject to Doppler effects; indeed they had only begun to suspect that it was a form of energy at all. But now that its wave structure has been detected by experiment, Doppler effects are to be expected in it, and reasons have been given for believing that the results of the Kaufmann and Michelson-Morley experiments are fulfilments of that expectation. Indeed it would appear that Einstein in his relativity equations made a great discovery, but, like that of Columbus, it was not the discovery that the discoverer supposed it to be. What Einstein supposed was a discovery about "space" and "time" was a discovery about the structure of matter. In the unit-changer which he selected for those dimensions, he stumbled upon the Doppler effects of a wave-constituted matter, just as Davisson in his experiments with the reflection of electrons from nickel crystals stumbled upon its diffraction effects. Thus the two discoveries confirm each other, a confirmation which is entirely lacking if we fail to recognize the physical cause back of Einstein's equations. If this account of things is correct, Einstein anticipated Davisson's discovery of the wave nature of matter by more than twenty years, but failed to appreciate what he had discovered because he failed to penetrate the dimensional disguise embodied in his own equations.

"But" it may be asked, "how did Einstein discover just what units to use to disguise Doppler effects so successfully? Did he follow the rule which was followed to discover the proper unit of length in which to disguise the law of Torricelli? Did he first derive the units from the law in order later to derive the law from the units in the circular manner described by Eddington?" The answer is that he did; not with any

intent to disguise or bewilder of course, but simply because it is often convenient for a mathematician to first work a problem backward in order to learn how to work it forward. The procedure is one frequently adopted, and it is a matter of record that Einstein's unit-changer was derived from the null result of the Michelson-Morley experiment—which is loaded with Doppler and aberrational effects—by a process of working backward from the result to the particular non-Newtonian units required to agree with it. The mathematics of the process cannot be followed here, but a general idea of how his units were derived from the experiment can be given. The experiment was performed with a Michelson interferometer mounted in a manner to be rotatable, and thus capable of varying its orientation with respect to the earth's motion. Two or more fully silvered, and one-half-silvered mirror are a part of the mechanism, and by reflection of light beams from these, Doppler and aberrational effects are produced in a manner which it is unnecessary to describe in detail here. Suffice it to say that they are produced by the earth's motion through space.[32] What the earth's absolute motion is—the motion relative to the ether—is unknown, but the average orbital motion around the sun is nearly nineteen miles a second. The beams of light which produce the observable result of the experiment are at right angles to each other and remain so as the interferometer is rotated, so that the Doppler and aberrational effects of the earth's motion always compensate or cancel out in the manner already explained. If on rotation of the interferometer the respective times taken by the two light beams to traverse their separate paths does not remain the same, the interference bands will shift, thus giving a positive result to the experiment, and revealing the earth's motion through space. If the time remains the *same* the bands will not shift and the result will be negative—as it turned out to be. Of the Doppler and

[32] As illustrating the ease with which these effects can be passed by unnoticed, we have Michelson's acknowledgment that he overlooked the aberrational effect of the earth's motion in his original calculations, for, speaking of the earliest trial of his famous experiment in 1881, he says:

"In deducing the formula for the quantity to be measured, the effect of the motion of the earth through the ether on the path of the ray at right angles to this motion was overlooked." (*Am. Jour. Sci.*, 3d Series, Vol. xxxiv, 1887, p. 334.)

aberrational effects involved in the experiment, all but one—the Lorentz contraction—were known to the physicists who designed the experiment and were allowed for in their calculations.[2] They were effects involving the first power of the earth's velocity; but the contraction is an effect involving the second power, and though very minute indeed, is detectable by the interferometer, and hence failure to allow for it misled the classical physicists. They were aware of the well known fact that Doppler and aberrational effects always cancel out when observed within the compass of a single coördinate system or frame of reference, but as they did not assume that their interferometer arms were composed of radiation, they did not allow for their contraction, and consequent change of length, in the direction of the earth's motion, and hence concluded that the experiment ought to give a positive result by revealing a shift in the interference bands when the interferometer was rotated. To their surprise it came out negative; there was no shift. And this is just the way the experiment should result if the radiation hypothesis is correct, since the second order Doppler effect cancels out, just as all other Doppler effects do. And of course the whole effect is observed from a single system, since light source, interferometer and observer are all on the earth and move with it.

Now Einstein took the null result of the Michelson-Morley experiment just as he found it, and in effect asked himself this question: How must I redefine the dimensions time and length (i.e. in what variable unit must I express them) in order that two interfering beams of light shall take the "same time" to traverse the paths (lengths) at right angles to each other which they do travel in the Michelson-Morley experiment, irrespective of the rotation of the interferometer? For if they always take the *same time* to traverse the paths irrespective of the rotation, the interference bands will not shift. Answering this question, he redefined the term "same time(ness)," or "simultaneity"—which merely means the quality common to zero time intervals—in the manner indicated in Sections 8 and 9 of his book "Relativity," and from this definition derived his two units in the manner indicated in Section 12. The principle of the constancy of velocity of light indeed, is stipulated in the course of deriving these units.

Now it is obvious that the result of the Michelson-Morley experiment is due to the operation of the laws of nature governing the behavior of light and matter in a system moving through space, and that the earth is such a system has been known since the time of Copernicus. Einstein derived his units for measuring time and length in such a manner that they successfully predicted the null result, since this is what he deliberately set out to do. He therefore *must* have made allowance for every Doppler and aberrational effect which occurs in the experiment—and there are many of them. He did not, however, as the classical physicists did, assume he already knew all the effects of this character which occurred. He deliberately avoided all appeal to such physical causes, but took the null result as he found it, without any hypothesis about the Doppler or aberrational effects of either first or second order, and obtained a dimensional expression in terms of time and length which allowed for them all—minus any physical theory about them. By this procedure he expressed the law of compensation of Doppler effects in his units, and hence was able to predict it from them—and it was by the operation of this law that the null result of the experiment was brought about. It is obvious that had any one of these effects *not* compensated the experiment would have given a positive result. Moreover, his new units enabled him to predict the effects when they do not compensate—as they do not when observed from systems in relative motion—and hence he was able to predict the positive as well as the negative results of Doppler effects. For the same law of nature that requires compensation when there is no relative motion, requires non-compensation when there is. Thus he first derived his units from the law and thus was able to derive the law from the units, but he could not have done this without the null result of the Michelson-Morley experiment to guide him.

When once we realize that matter like light is a form of radiation, the results of the Michelson-Morley and Kaufmann experiments are indicated to be consequences of Doppler effects, and hence to fall into the same class as the results of experiments revealing the Doppler effect, the aberration of

light, the Fizeau effect and the Airy effect. They both become phenomena inferable from Roemer's discovery that radiation has a finite velocity, and hence allowance must be made for change of motion of its radiator or receptor—whether sun or electron. This contention the relativists only *appear* to challenge when they deny the Doppler effect which results from that change—for they do not mean what they appear to mean. *They allow for the change as well as the classical physicists, only they make the allowance through axiomatic unit-changers instead of in the classical manner.* It is only relative to the ethereal medium that the velocity of light is always constant. It is *this* velocity which is independent of the velocity of material bodies in general, including bodies which emit or absorb it. If radiation had an infinite velocity, as assumed previous to Roemer, or if it had a constant Newtonian velocity relative to its source or sink, Doppler phenomena could not exist, and hence there would be no relativity theory. The whole supposed controversy between the relativists and non-relativists arises from a misunderstanding due to ambiguity of words, for to assert that the red-haired *non-Newtonian*, "velocity" of radiation relative to its source or sink does *not* change is to assert that the black-haired *Newtonian* "velocity" *does* change, and from that change arises a corresponding change in the physical cause back of the relativity equations.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

STUDIES OF CAST RED BRASS.

At the recent annual conference of British Foundrymen held during June, the "American Exchange Technical Paper" was one by C. M. Saeger, a member of the Bureau's metallurgical staff. The preparation of the exchange paper is an honor bestowed annually on some prominent foundry metallurgist by the American Foundrymen's Association. This year's paper entitled "Studies on Cast Red Brass for the Establishment of a Basic Classification of Non-Ferrous Ingot Metals for Specification Purposes" summarized work done in coöperation with the Non-Ferrous Ingot Metals Institute. It dealt with the alloy known as cast red brass, containing 85 per cent. copper, 5 per cent. each of lead, tin and zinc—one of the most important copper-base alloys used in the non-ferrous foundry industry, and covered an immense amount of laboratory study on factors which determine the suitability of this material as a standard for specification purposes, such as the proper method of casting testing bars, temperature of pouring the alloy, impurities such as sulphur and iron, etc.

STRESS-STRAIN DETERMINATIONS ON STEEL WIRE.

Stress-strain determinations on steel wire intended for bridge cables have shown that steel wire hardened by cold drawing assumes a greater permanent elongation under like values of tensile stress than does steel wire of similar composition hardened by heat treatment so as to have the same ultimate tensile strength. Under the test conditions used, the limit of proportionality of strain to stress for cold drawn wire seemed to be influenced to a greater degree by the time during which the tensile stress was applied than was the case for heat-treated wire.

PRESERVATION OF RECORDS IN LIBRARIES.

The results of systematic studies at the Bureau, extending over a period of four years, of the problems of preserving

* Communicated by the Director.

valuable records in libraries, were summarized in a report presented by B. W. Scribner, chief of the paper section, at a meeting of the American Library Association, in Montreal, on June 28. The report was given at a session of the Association's public documents committee, which is devoting particular attention to this important subject.

The Bureau's studies on the relation of light, adverse temperature and humidity, acid pollution of the air, and low-grade paper to deterioration of paper in libraries were described and the means developed for minimizing deterioration from these causes were explained. These studies were made with the assistance of a fund granted by the Carnegie Foundation to the National Research Council, but were discontinued a short time ago because of the depletion of this fund and of the Bureau's appropriation available for the purpose. A resume of the results obtained was published in the July *Library Quarterly*, and a detailed summary is contained in the Bureau's Miscellaneous Publication M144, copies of which are obtainable from the Superintendent of Documents, Government Printing Office, Washington, D. C., at 5 cents each. In Mr. Scribner's paper, the following were mentioned as problems which should be studied to round out the partial information already obtained: The elimination of odors and the efficacy of air filters in purification of air; the kind of light rays most harmful to paper and means for eliminating such rays in library illumination; protective and reproductive processes for records contained on impermanent papers; prolonging the life of the lower-grade papers by incorporated materials; the effect of acidity from perspiration; the causes of "foxing" and molding of paper; and the repair of books, manuscripts, and other forms of records.

ROOF EXPOSURE TESTS OF STRAIGHT RED LEAD PAINTS.

In Technical News Bulletin No. 207 (July, 1934) a short note is devoted to outside exposure tests being made at the Bureau on red lead paints. In addition to these mentioned, a second series was started (January 31, 1929) at about the same time, in which the red lead was increased from 40, 50, 60, 70, 80, 90, up to 100 lbs. red lead to a gallon of linseed oil. Beyond 50 lbs., it was necessary to add thinner (turpentine)

in increasing amounts, in order to make the paints brushable. The purpose of the test is to find the "critical ratio" of pigment to nonvolatile binder (oil), at which the most durable red lead paint is obtained. Paints below this critical ratio will have too much oil in proportion to the red lead. Likewise paints above this critical ratio will be deficient in oil. After 5½ years of exposure, the critical ratio has not, as yet, been obtained. There is some evidence that the panels containing 40 lbs. red lead to a gallon of oil are among the best.

RUBBER FROM GOLDENROD.

Some time before the death of Edison, the newspapers contained many comments on his plans to obtain rubber from goldenrod. It is not surprising that rubber occurs in this plant, because it is found in others in the same family, notably in guayule, which is an important commercial source. The Bureau of Plant Industry of the Department of Agriculture has taken over the work of the company organized by Edison, and will at first attempt to produce a race of the goldenrod that contains a higher percentage of rubber than the average wild plants. In order to find the most promising material upon which to work, determinations of the rubber content of several thousands of individual plants must be made. The National Bureau of Standards has been asked to coöperate to the extent of checking two methods which are now in use for determining the rubber content. One method takes a comparatively long time, and only a few analyses can be made in a day, but it is believed to be accurate because the rubber is weighed directly. The other method is rapid and by it a chemist can make as many as thirty analyses in a day. However, the rubber is not weighed, but is converted into a bromine compound. The amount of rubber is calculated by an empirical factor, from the quantity of bromine that enters into combination. The chief task of this Bureau will be to check the values of this factor.

Wooden Chips.—(*Ind. and Eng. Chem.*, News Edition, **12**, 263.) L. A. Helwich, in his Czechoslovakia letter, tells of a recently developed wood-gas generator which may be applied to all internal combustion engines. In these generators 27 ounces of wood are claimed to be the equivalent of one liter of gasoline.

C.

Tungsten Crystals for Light Bulbs.—(*Ind. and Eng. Chem.*, News Edition, **12**, 264.) The Osram Co. in Prague, Czechoslovakia, has installed a process for the manufacture of recrystallized tungsten wire. The wire is continually drawn over a zone of above 2000° C. by a velocity exceeding that of the crystal growth. The metal is brought to a white heat for a brief period and the wire is continually transformed into one long crystal.

C.

Grapefruit Oil.—(*Ind. and Eng. Chem.*, June, 1934.) According to E. K. Nelson and H. H. Mottern of the U. S. Department of Agriculture there is more to a grapefruit than the juice that meets the eye. Approximately 100,000 tons of grapefruit are processed annually, the peel from half of which is conservedly estimated as available for the manufacture of oil. On the basis of a commercial yield of one pound per ton of fruit, 50,000 pounds of oil could readily be produced if the market should call for this quantity. The oil is new to commerce but has recently won favor in England where it is used for flavoring beverages.

Grapefruit oil as prepared by the Pipkin process is an amber colored liquid of pleasing odor resembling both orange and lemon oils. Its contains approximately 90 per cent. limonene, 7 to 8 per cent. waxy materials and small quantities of alcohols, aldehydes and terpenes.

H. D. Poore, also of the U. S. Department of Agriculture, announces that complete analyses of grapefruit residues such as membrane, peel and seeds, show between 3 and 4.5 per cent. of pectin and about three-quarters of a one per cent. of naringin to be present. Pectin, as most everyone knows, is a constituent of practically all fruit juices and is responsible for their jelling. Naringin is classed as a glucoside in that, like many plant products, it represents a combination of a simple sugar with other organic chemical compounds. Naringin has not been found in the flowers or fruit of any other variety of citrus. It is quite bitter to the taste and 1 part dissolved in 50,000 parts of water can be detected.

C.

THE FRANKLIN INSTITUTE

LIBRARY NOTES.

RECENT ADDITIONS.

Aircraft Year Book for 1934. Volume 16.

BARTON, SAMUEL G., AND WM. H. BARTON. A Guide to the Constellations. 1928.

Beilsteins Handbuch der organischen Chemie. Vierte Auflage. Erstes Ergänzungswerk die Literatur von 1910–1919 um fassend. Herausgegeben von der Deutschen Chemischen Gesellschaft. Bearbeitet von Friedrich Richter. Fünfzehnter und sechzehnter Band. 1934.

Eastman Kodak Company. The Photography of Colored Objects. Twelfth Edition (Revised). 1933.

Eastman Kodak Company. Photomicrography. An Introduction to Photography with the Microscope. 1932.

ENGELHARDT, VICTOR. Handbuch der technischen Elektrochemie. Band 1, 1–3 Teilen; Band 2, 1–2 Teilen. 1931–1933.

Eucken, A., AND K. L. WOLF. Hand-und Jahrbuch der chemischen Physik. Band 1, Abschnitt 1; Band 3, 1 Teil, Abschnitt 1; Band 4; Band 5, Abschnitt 3; and Band 9, Abschnitt 1. 1932–1934.

FREEMAN, HENRY. English-German Dictionary for Metallurgists. (Ferrous and Non-Ferrous Metallurgy.) Part II, English-German. 1934.

FRODSHAM, CHARLES. A Few Facts Connected with the Elements of Watch and Clock Making. 1862.

FRODSHAM, CHARLES. A History of the Marine Chronometer; with the Laws of Isochronism of the Balance Spring, as Connected with the Higher Order of Adjustments of Watches and Chronometers. 1871.

HODGMAN, CHARLES D., Editor. Handbook of Chemistry and Physics. A Ready-Reference Book of Chemical and Physical Data. Eighteenth Edition. 1933.

HOLMES, HARRY N. Out of the Test Tube. 1934.

MCCORKLE, PAUL, AND J. ARTHUR LEWIS. College Physical Science. An Orientation Course. 1934.

National Research Council. Annual Survey of American Chemistry. Volume VIII, 1933. Edited by Clarence J. West. 1934.

NERNEY, MARY CHILDS. Thomas A. Edison, a Modern Olympian. 1934.

Patterson's American Educational Directory. Volume XXXI, 1934.

RHODES, J. E. WYNFIELD. Phase Rule Studies: an Introduction to Phase Theory. 1933.

SMITH, DAVID EUGENE, AND JEKUTHIEL GINSBURG. A History of Mathematics in America before 1900. Carus Mathematical Monograph no. 5. 1934.

STEVERS, MARTIN D. Steel Trails: the Epic of the Railroads. 1933.

TURCK, J. A. V. Origin of Modern Calculating Machines. A Chronicle of the Evolution of the Principles that Form the Generic Make-Up of the Modern Calculating Machine. 1921.

WALKER, MILES. Conjugate Functions for Engineers. A Simple Exposition of the Schwarz-Christoffel Transformation Applied to the Solution of Problems Involving Two-Dimensional Fields of Force and Flux. 1933.

WILTERDINK, J.-H. Sur un Régulateur de Léon Foucault. Parfaitement Isochrone et de Construction Simple et Sur une Tentative de Rendre cet Appareil Indépendant de l'Influence de la Température. 1918.

World Power Conference. Sectional Meeting. Scandinavia. 1933. Transactions. Volumes 2, 3, 4 and 7. 1934.

BOOK REVIEWS.

ELEMENTARY QUANTITATIVE ANALYSIS, THEORY AND PRACTICE, by Hobart H. Willard, Ph.D., Prof. of Analytical Chemistry, University of Michigan, and N. Howell Furman, Ph.D., Associate Prof. of Chemistry, Princeton University, 406 pages, illustrations, tables, 14 × 22.5 cms. New York, D. Van Nostrand Company, Inc., 1933. Price $3.25.

In their "Preface," the authors state they have endeavored to include all of the theory and laboratory procedures required for the successful conduct of the first year's work in Quantitative Analysis. As the basis for an introductory course, the volume deals with both the theory and practice of volumetric and gravimetric methods as well as an introduction to electroanalysis. The method of presentation conforms pretty well to the generally accepted ideas of instruction. Much emphasis has been placed upon the theoretical principles upon which the quantitative methods are founded. The theory of each type of method is discussed immediately before in connection with the laboratory procedure. Recognizing the modern tendency of teachers to give volumetric methods priority over gravimetric, the authors have dealt with these two main divisions of quantitative analysis in the order stated.

Chapter One, as the "Introduction," contains a general summary of Quantitative Analytical Methods including those which involve Physico-Chemical Measurements. A bibliography of books upon analytical chemistry is furnished for the benefit of students desiring to pursue the subject further, as well as for enhancing the book's reference value. The second chapter, on Fundamental Laws and Theories, expounds the Law of Mass Action and Electrolytic Dissociation. This is followed by a discourse on general laboratory methods and technique.

Chapters IV to XII, inclusive, contain the general and detailed instructions on volumetric analytical methods. Careful attention is paid to the matter of precise weighing and accurate calibration of apparatus. The three principal volumetric procedures are taken in order. These are Acidimetry and Alkalimetry, or Neutralizations; Volumetric Precipitation Reactions; and Oxidations and Reduction Processes. The chapter on the Theory of Indicator Action and that of Neutralization are particularly commended to the student's careful reading and

study. The volumetric section is terminated with a very complete summary of volumetric reactions expressed in the equation form.

Chapters XIII to XVIII cover the gravimetric methods. The opening chapter on the Theory, is extremely interesting and enlightening as well, discussing as it does, the conditions for complete precipitation and the purity of the precipitate. Description of actual gravimetric analyses follow preliminary instructions on precipitation, filtration, washing of precipitates, drying and ignition, and calculations. After describing the simpler methods, more involved procedures of gravimetric separations are illustrated by the analysis of limestone and of an insoluble silicate.

The method of electrolytic precipitations and separations is introduced by an adequate discussion of the theory followed by directions for the practical determination of copper, zinc and the analysis of brass, copper-nickel alloys, and German silver.

An Appendix contains tables of Ionization Constants and Solubility Products. At the end of almost every chapter will be found a group of review questions and problems.

T. K. CLEVELAND.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS.

Report No. 481, Working Charts for the Determination of Propeller Thrust at Various Air Speeds, by Edwin P. Hartman, 28 pages, illustrations, tables, 23 × 29 cms. Washington, Superintendent of Documents, 1934. Price ten cents.

A set of propeller performance charts, based on a "torque speed" coefficient C_{QS}, has been constructed from full-sized metal propeller data obtained in the N.A.C.A. propeller-research tunnel.

The simplifying assumption that the torque of the engine remains constant (which appears to be nearly true except for highly supercharged engines) is utilized in the application of the coefficient C_{QS} which is defined as follows:

$$C_{QS} = \frac{V}{nD}\sqrt{\frac{1}{C_Q}} = V\sqrt{\frac{\rho D^3}{Q}}$$

from which the troublesome variable n (engine revolution speed) has been eliminated. The coefficient C_{QS} when plotted against the ratio C_T/C_Q, which is equal to TD/Q, provides a useful chart from which propeller thrust may be quickly calculated for any air speed. Twenty-four tables are included to present the data from which the charts previously published in Technical Report No. 350 were obtained.

Report No. 477, Effect of Viscosity on Fuel Leakage Between Lapped Plungers and Sleeves and on the Discharge from a Pump Injection System, by A. M. Rothrock and E. T. Marsh, 17 pages, tables, illustrations, 23 × 29 cms. Washington, Superintendent of Documents, 1934. Price ten cents.

Test data and analysis show that the rate of fuel leakage between a lapped plunger and sleeve varies directly with the density of the fuel, the diameter of the plunger, the pressure producing the leakage, and the cube of the mean clear-

ance between the plunger and sleeve. The rate varies inversely as the length of the lapped fit and the viscosity of the fuel. With a mean clearance between the plunger the sleeve of 0.0001 inch the leakage amounts to approximately 0.2 per cent. of the fuel injected with gasoline and as low as 0.01 per cent. with diesel fuel oils. With this mean clearance an effective seal is obtained when the length of the lap is three times the diameter of the lap. The deformation of the sleeve and plunger under pressure is sufficient to change the rate of leakage appreciably from that which would be obtained if the clearance was constant under pressure. The rates of fuel injection with a commercial fuel-injection pump showed little variation for a range of fuel viscosities from that of gasoline to that of S.A.E. 30 lubricating oil.

PHYSICAL OPTICS, by R. R. Wood, Hon. Ph.D. (Un. of Berlin); LL.D. (Edinburgh); Foreign Mem. Royal Soc. (London); Acad. dei Linceé (Rome); Professor of Experimental Physics in the Johns Hopkins University; xvi + 846 pages, 15 × 23 cm., cloth, third edition. New York, The Macmillan Company, 1934. Price, $7.50.

There are few treatises that combine at once the qualities of logical deduction, analytical acumen and practical technique that characterize "Physical Optics." It made its first appearance in the early nineteen hundreds, a period when searching investigations had more than hinted at the changes in the fundamental concepts of physical science that are now in progress. From its first appearance the book has justly been regarded as a compendium of worth-while information in tangible form, for Professor Wood always describes how an experiment may be performed by the simplest means when that is possible, and he often makes it possible.

The present edition bears ample evidence of the profound changes which have taken place by the large amount of new matter which has replaced that in the previous edition. Practically, the work has been rewritten to bring it in accord with modern fundamental concepts. The new edition, the author states, has 827 pages, an increase of 132 pages, of which 495 pages is new material. The illustrations and diagrams are numerous and satisfying, and the color plates instructive and beautiful, but no printed page can duplicate the striking effect of projected spectral colors.

The work remains well in the van of progress in physical science.

L. E. P.

THE CHEMISTRY OF PETROLEUM DERIVATIVES, by Carleton Ellis, 1285 pages, tables, 16 × 23.5 cms. New York, Chemical Catalog Co., Inc., 1934. Price $18.00.

This work might just as properly be labelled: Modern Aliphatic Chemistry. It is written by a specialist in that particular subject and is intensely practical in scope. The size and number of pages should convince the examiner that the author has included virtually everything having to do with chemistry of petroleum derivatives. Every student of organic chemistry can appreciate the difficulties encountered by chemists doing research on this family of hydrocarbons. The establishment of a chemical industry based on petroleum products and comparable to that fostered by coal-tar does not await so much the development of synthetic

chemical processes but rather refinements in physico-chemical methods which shall expedite the separation and isolation of such derivatives as are already present in their natural state. Once this is done, the chemist's task is relatively easier since the synthetic procedure against any individual compound has been worked out quite thoroughly.

According to the author, "every effort has been made to make it complete and to include in it references to all published material of both purely scientific and industrial interest as well as to the vast amount of information contained in patents on the subjects." The decision to publish this accumulation of material is indeed timely, in more ways than one. A delay of another year or two might have meant two volumes instead of the present single one.

Almost every imaginable phase of aliphatic chemistry is discussed. The presentation of the subject material follows a logical course. The mechanism of "cracking" and the various methods of doing such lead off the parade, followed by a discussion of the formation of hydrogen and carbon, a consequence of carrying "cracking" too far. The olefins take up considerable space. This is reasonable because they are highly reactive chemically and serve as convenient starting points in the preparation of alcohols, esters, ketones, chlorinated solvents, synthetic rubber, fumigants, and intermediates in the manufacture of many fine chemicals. Oxidation processes applied to paraffin hydrocarbons promise large returns in a variety of desirable oxygen-containing products. Other reagents such as sulphuric and nitric acids, nitrosyl chloride, et cetera, receive their share of attention. Two very practical chapters terminate this enormous reference work. They deal with the "Analysis of Natural Gas and Cracking Gas"; and "Methods of Analysis of Gasoline and Other Petroleum Distillates." A Name and Subject Index makes the job complete.

T. K. CLEVELAND.

TRAITÉ DE GÉODÉSIE, by P. Tardi, xxx and 732 pages in 2 fasciculi, illustrations, diagrams 17 × 25 cm. Paris, Gauthier-Villars, 1934. Price 150 francs.

Among French geodesists a treatise on geodesy by Francoeur was an old stand-by for generations. The first edition was published nearly a century ago, in 1835. Successive revisions changed it and added to it for generations. Not long ago the last printing of the last edition of Francoeur was exhausted and the question arose whether to revise still further or to start afresh. The latter course was chosen. The changes, not to say the revolutions, due to new inventions, most of them subsequent to the date of the last revision of Francoeur, made a complete restatement necessary. A mere list of some of the more striking recent introductions will suffice; invar tapes or wires instead of bars for measuring bases; the impersonal micrometer; the demountable signal; the radio; the torsion balance, the Meinesz apparatus for determining gravity at sea. The author adds another item, the prismatic astrolabe, which is hardly known on this side of the Atlantic.

The author is well qualified for his task. He has had much field experience in the resurvey of the French war zone and in the survey of the various French colonies. He has worked on the International Bibliography of Geodesy and was a member of a special geodetic mission to Roumania.

The book resembles a good American textbook in geodesy but is somewhat more advanced. Even apart from the difficulty arising from the foreign language,

it is thus hardly a book for a beginner. One who already knows something of geodesy can read it with profit. It is a book for not only the engineer in the field but for the student of theory. The latter phase is necessarily treated somewhat slightly owing to limitations of space.

Two matters may be singled out for special mention: the subject of plane coördinates and the attention paid to physical geodesy.

Plane coördinates for topographic and cadastral purposes have been known for over a century. Various systems were used by the different states of Germany and many of the countries of Europe have adopted one or more systems according to the extent of their territory. Since an ellipsoidal surface, like that of the earth, can not be exactly represented on a plane, the territory covered by a single system of plane coördinates must be restricted in order to keep the deformations within tolerable bounds. The U. S. Coast and Geodetic has devised a set of systems covering the entire country and such that the deformations are always small. Some states can be served by a single system; others need several. Two states have adopted these systems officially and the use of them is spreading in others. Tardi's book gives a good introduction to the general subject; it was published just too early to mention even early developments of the idea of plane coördinates in this country.

When the geodesist undertakes to interpret his results he becomes thereby a geophysicist. This condition has always prevailed, even before anyone invented the word "geophysics," but the connection between geodesy and the other branches of geophysics (the phrase is used advisedly) is now closer than ever. Tardi's book brings out this connection without, however, going very far into the subject. Probably future American textbooks will pay more attention to the geophysical aspects of their subject.

The book has already been used in the U. S. Coast and Geodetic Survey and in the process several typographical errors were detected, mostly of a kind easily detected and rectified.

WALTER D. LAMBERT.

THE CONDUCTION OF ELECTRICITY THROUGH GASES, by K. G. Emeleus with a general preface by O. W. Richardson. 94 pages, illustrations, 10 × 17 cms. New York, E. P. Dutton and Co. Price $1.10.

A member of a series of Monographs on Physical Subjects, this handy little volume gives an outline of the main phenomena which can be studied quantitatively in connection with the passage of electricity through gases at low pressures, in particular those associated with the glow-discharge. A work of this nature is intended to be a distinct help to students of physics. Not only that, but it makes a handy reference volume on a subject, in regard to which, trained physicists and teachers of physics often wish to refresh their memory.

The subject material is presented in as simplified a manner as possible with frequent illustrations to clarify those portions of the text least easy to visualize. The topics fall into six main divisions: (1) Initiation of the Discharge; (2) Cathode Phenomena; (3) Exploring Electrodes; (4) the Negative Glow, Faraday Dark Space, and Anode Glow; (5) the Positive Column of Glow-Discharges; and (6) Miscellaneous Phenomena. The material under the above headings is further subdivided into topics, each one more or less complete in itself.

T. K. CLEVELAND.

SIGNALS AND SPEECH IN ELECTRICAL COMMUNICATION, by John Mills. 281 pages, 13 × 19 cms. New York, Harcourt, Brace & Company, 1934. Price $2.00.

Do you know how a telephone can be induced to howl and why it should be that way in the first place? Have you ever wondered just what inventions finally extended the limits of long distance telephony to include direct communication between New York and San Francisco where previously Denver was the western boundary? Do you realize that the telephone "dial system" represents the acme in development of electrical brains? Have you heard why politeness is not the only good reason for not interrupting the speaker during a long distance conversation? These are merely a smattering of the many questions projected by the laity on the subject of electrical communication and answered by the author's series of essays.

The material is presented in such a manner that the treatment of each topic forms a sort of groundwork leading to a better understanding of the subject directly following. The author is a trained scientist and is exceedingly well versed in his subject. Moreover, he has had considerable experience as a teacher and to reach the non-scientific reader, has followed the precepts of Mohammed in coming to the mountain. Done in this manner, the book is exceptionally free of those scientific abstractions so essential in text books. Rather, the book represents an interesting exposition on electrical communication with special care paid to the romance of its development, all of which contributes to produce a volume of effortless reading.

The author starts with the Vivisection of Speech. Here as in other instances, ether plays an important role. A description of the dial system of telephoning is aptly described as communication with Electrical Brains. Frequencies and Modulation are fundamental topics and precede a discussion of such interference phenomena as Action and Reaction, Attenuation and Echoes. Following the chapters on the characteristics of electrical communication circuits, the various forms of apparatus receive their share of attention. Transmitters and receivers now bear slight resemblance to the originals both in respect to form and performance. The vacuum tube has won the title of "Modern Jinn." There is a timely essay on the "talking pictures" and the difficulties of television are expertly balanced against the possibilities for the future. The cathode ray tube offers renewed hope.

If the reader has wondered how as many as twelve different telegraph messages can be transmitted over a single pair of wires the chapter on Filters and Channels will enlighten him considerably. Under Levels of Communication the "noise-meter" and its decibel measuring ability is discussed in relation to the part it can play in the control of sound intensities best suited to a particular type of communication. All this induces one to look eagerly forward to what the future may bring in the line of advances in electrical communication.

T. K. CLEVELAND.

LIQUID DIELECTRICS, by Dr. Andreas Gemant. English translation by Vladimir Karapetoff. 185 pages, illustrations. 16 × 23 cms. New York, John Wiley and Sons, Inc., 1933. Price $3.00.

Liquid Dielectrics is the second of a series of monographs being published under the auspices of the Electrical Insulation Committee, National Research

Council. The chief function of the book is intended to be utilitarian and the author has in many instances drawn from his own practical experiences acquired in the manufacture of electric cables. The importance of liquids as insulating media is well recognized by the electrical engineering profession. The nature of the book is such that the engineer may readily grasp the significance of the facts set forth. Emphasis has been placed upon those physical characteristics of liquids which are closely related to dielectric behavior. For the same reason the electrical and electro-optical sections have been treated more elaborately. In order to keep the practical side of the subject clearly before the reader at all times, much of the related theoretical material with its mathematical development has been treated as briefly as possible. A very helpful feature is the author's practice of concluding each topic with a summarizing paragraph.

Under the heading of Essential Mechanical and Thermal Properties, capillarity, viscosity and conduction of heat are adequately discussed. Solubility and gas absorption, colloidal structure, and oxidation are classified under physico-chemical behavior while a discussion of electrical and optical properties includes, conductivity with direct current, dielectric constant, index of refraction, dielectric loss and the Kerr effect. A chapter on the behavior of the liquid dielectrics in intense fields is of the greatest practical importance. Here are given the probable mechanism of the chemical changes; a description of the ionization phenomena and the causes of the "breakdown." A concluding chapter deals with the application of liquid dielectrics in electrical engineering. This treatment is particularly impressive backed as it is by the author's personal experience with high voltage cables, transformers, circuit-breakers and other liquid electro-phenomena. At the end of the book, an appreciable list of literary references will be found, classified according to the separate sections of the book.

T. K. CLEVELAND.

PUBLICATIONS RECEIVED.

Achema VII, Ausstellungsführer. Ein Bezugsquellennachweis Chem. Apparate und Maschinen für Wissenschaft und Technik. Herausgegeben von der Achema. 124 pages, illustrations, plates, 15 × 21 cms. Köln, 1934.

Encyclopédie Photométrique, Tome IV, Photométrie des Lumières Brèves ou Variables, par Madame Marguerite Moreau-Hanot, preface de M. Charles Fabry, 126 pages, tables, illustrations, 16 × 24.5 cms. Paris, Revue d'Optique theorique et instrumentale, 1934. Price 25 francs.

Organic Chemistry, or Chemistry of the Carbon Compounds, by Victor von Richter, edited by Prof. Richard Anschutz and Dr. Fritz Reindel. Volume I, Chemistry of the Aliphatic Series, 790 pages, tables, 15 × 23 cms. London, Kegan Paul, Trench, Trubner & Co., Ltd. Philadelphia, P. Blakiston's Son & Co. Price $10.00.

Arsenical and Argentiferous Copper, by J. L. Gregg, with a Foreword by H. Foster Bain, 189 pages, illustrations, tables, 15.5 × 23.5 cms. New York, The Chemical Catalog Company, Inc., 1934. Price $4.00.

A Textbook of Organic Chemistry, by Joseph Scudder Chamberlain, 873 pages, illustrations, 15 × 22 cms. Philadelphia, P. Blakiston's Son & Co., Inc. Price $4.00.

Electrons at Work, A Simple and General Treatise on Electronic Devices, Their Circuits, and Industrial Uses, by Charles R. Underhill, 354 pages, illustrations, 15 × 23.5 cms. New York and London, McGraw-Hill Book Company, Inc., 1933.

Faraday, by Thomas Martin, 144 pages, 11.5 × 19 cms. London, Gerald Duckworth & Co., Ltd. Price 2 shillings.

Republica de Panama, Secretaria de Agricultura y Obras Publicas, Censo Demografico, 1930, 2 volumes, 17.5 × 25 cms. Comision General de Reclamaciones, Registro Nos. 10, 19, 24, 25, 16 × 24 cms. Panama, Imprenta Nacional, 1934.

Bell Telephone Laboratories, Monograph B-792, Late Developments in Microscopy, by Francis F. Lucas, 47 pages, illustrations, tables, 15 × 23 cms. New York, Bell Laboratories, 1934.

U. S. Bureau of Mines, Gold and Silver, by J. P. Dunlop, 24 pages, 15 × 23 cms. Washington, Government Printing Office, 1934. Price five cents. Ore Concentration, Metallurgical Results and Flotation Reagents, by T. H. Miller and R. L. Kidd, 13 pages, 15 × 23 cms. Washington, Government Printing Office, 1934. Price five cents.

National Advisory Committee for Aeronautics, Technical Notes No. 446 (Appendix) Calculation of Horsepower Available, by Shatswell Ober, Appendix to Estimation of the Variation of Thrust Horsepower with Air Speed, 2 pages, 20 × 26 cms. Washington, Committee, 1934. No. 499, Effect of Retractable-Spoiler Location on Rolling and Yawing-Moment Coefficients, by J. A. Shortal, 12 pages, tables, illustrations, 20 × 26 cms. Washington, Committee, 1934. No. 500, The Torsional Stiffness of Thin Duralumin Shells Subjected to Large Torques, by Paul Kuhn, 8 pages, illustrations, 20 × 26 cms. Washington, Committee, 1934. No. 501, Landing-Shock Recorder, by M. J. Brevoort, 8 pages, tables, illustrations, 20 × 26 cms. Washington, Committee, 1934. Report No. 482, Wing-Fuselage Interference Tail Buffeting, and Air Flow about the Tail of a Low-Wing Monoplane, by James A. White and Manley J. Hood, 21 pages, illustrations, 23 × 29 cms. Washington, Superintendent of Documents, 1934.

CURRENT TOPICS.

Sulfur.—The Greeks had a name for it: Theion. The Romans were more modern: They called it "sulfur." This information, and more, are to be found in a recently published booklet by the Texas Gulf Sulphur Company giving the history, occurrence, mining, uses, and properties of that mythological Inferno fuel, popularly known as "brimstone."

It seems that linen was bleached with sulphur as early as 2000 B.C. and certain Egyptian paintings of about 1600 B.C. contain colors that require sulphur compounds. Homer refers to its employment as a disinfectant, and Dioscorides, according to his materia medica, knew of its dermatological value when applied in the form of sulphur ointments. During the time of the Caesars sulphur was used in the preparation of incendiary materials of warfare. The Romans also utilized sulphur in casting many of their bronze triumphal statues, in order to obtain the desired finish.

In general, free or native sulphur occurs in three types of deposits:

1. *Solfataras.*—The sulphur deposits associated with solfataras, or volcanic vents, are formed by the oxidation of hydrogen sulphide. The sulphur so produced is deposited in the tufas or other adjoining porous rock as in the deposits of Japan and Chile.

2. *Mineral Springs.*—Fumarole deposits, which may be seen incrusting hot springs, are the result normally of the interaction of sulphur dioxide and hydrogen sulphide.

3. *The Gypsum Type.*—This is by far the most important group of deposits because it supplies over 95 per cent. of the world's sulphur. Here the sulphur occurs in pure crystals disseminated through limestone and gypsum. The enormous deposits of the Gulf Coast of the United States and the deposits of Sicily are of this type.

The Italian and Sicilian sulphur deposits are very similar to those in this country and in many cases, equally modern methods of working them are in effect. The sulphur deposits of Japan are all of volcanic origin and since many of the vents or fumaroles are still active, a small quantity of sulphur is recovered by passing the escaping gases through condensers. Ordinarily the sulphur is distilled from the ore by heating in closed retorts. Chilean sulphur deposits come high, being of volcanic origin and found at altitudes ranging from 13,000 to 20,000 feet. Rarity of the atmosphere, cold

and violence of the prevailing winds make operations exceedingly difficult.

Since 1905 practically all the sulphur used in this country has come from Louisiana and Texas, where the deposits occur in the cap rock overlying certain salt domes at depths ranging from 500 to 1500 feet below the surface. The sulphur is mined by the Frasch process. This consists in first drilling a well into the sulphur formation. Three concentrically arranged pipes, with diameters of 6 inch, 3 inch and 1 inch, respectively, are then inserted and reach to the bottom of the ore body. Hot water is pumped down the annular space between the 6-inch and 3-inch pipes. The water circulates through the porous formation, melting the sulphur which makes its way downward, forming a pool at the bottom of the well and rising up into the 3-inch pipe. If, now, compressed air is forced down the central 1-inch pipe the molten sulphur is lifted up the 3-inch to the surface.

Sulphur has no action on the skin, but some of it may be converted into hydrogen sulphide which acts as a mild vascular stimulant. If taken internally there is a laxative effect, owing perhaps to the small amount of hydrogen sulphide produced in the intestines. Sulphur belongs to the group of materials that are poor conductors of electricity. Owing to the fact that it adsorbs less moisture, its surface resistivity is better than that of most materials at 50 per cent. and 90 per cent. relative humidity. Sulphur dust and air, if in a confined space, constitute an explosive mixture. The latter may be detonated by a discharge of static electricity or a spark caused by metal striking against metal or stone.

Sulphur as such is a rather brittle, friable material; but when mixed with an aggregate such as sand, it may be used in the production of cements. Sulphur may be used to impregnate wood, stone, fiber board, etc., to increase their strength and improve their moisture-resisting and electrical properties. Maple may be impregnated with 52 per cent. of sulphur and its modulus of rupture increased from 17,000 lbs./sq. in. to 33,500 lbs./sq. in. A dense sandstone may be impregnated with 10 per cent. of sulphur and its compression strength increased from 9000 lbs./sq. in. to 30,000 lbs./sq. in.

C.

Rheology and Philadelphia.—Most persons know what Philadelphia is but only a few are acquainted with Rheology. Having its root in the Greek word "Reos" meaning "anything flowing" or a "stream," rheology represents the science of flow. Not so many years ago the Society of Rheology was formed having for its mem-

bers scientists and engineers who are intimately concerned with the flow characteristics of a great variety of substances. Its guiding officers have served to maintain the Society in a vigorous condition and now all Philadelphians interested in rheological phenomena will have an opportunity to view at close range, the workings of the Society.

The Sixth annual meeting of the Society of Rheology is to be held in Philadelphia, Pa., at the Franklin Institute on Friday and Saturday, October 19th and 20th, beginning at 9 A.M. Everyone is invited to attend these meetings whether a member or not.

C.

Lightning and Golf.—(U. S. D. A. Clip Sheet No. 838.) Speaking of golf greens it is reported that lightning made a hole in one at a golf course near Washington, D. C. The thunderbolt, striking on an elevated putting green, left a hole of small diameter at least 43 inches deep. From this hole two branches ran down the slope, while a third ran up onto the green, where it forked several times. A narrow path of seared grass plainly marked its course.

C.

Sterilizing Water with Silver.—(*Chem. and Met.*, Vol. 41, p. 372.) The Katadyn process, introduced into this country from Germany, is an electro-chemical method for the sterilization of water by treatment with silver. Water so treated takes on bactericidal properties which persist as long as silver remains in solution. The action of the silver is not immediate but requires from 30 minutes to several hours. Most important of all, the quantity of silver is so small that it is without any harmful effect to the human system, and the treatment does not impart any objectionable smell, taste or color to the water.

The silver may be brought into solution by either the contact method or the electrolytic method. The former has the disadvantage of being limited to water free from suspended matter. For treating large quantities of water the electrolytic method therefore is used. The activator consists of an iron tank, lined with a special insulating material, in which are placed electrodes of silver. Polarization is prevented by automatically reversing the current at definite intervals. The energy consumption will seldom exceed one watt-hour per ton of water treated. Cost of silver is about 1 mill per ton of water for drinking purposes but may run as high as 1 to 1½ cents for highly bactericidal rinsing water to be used in food and beverage plants. Drinking water may consume 25 to 100 milligrams of silver per ton.

Successful installations in Germany at this time include a purification plant for drinking water near Heidelberg; a swimming pool at the Frankfort stadium, an ice plant in Dresden and a brewery in Munich. The new German Navy cruiser, Königsberg, has had an electrolytic plant installed. In this country, a purification unit has been installed in the new 150,000 gallon swimming pool at the Congressional Country Club, Washington.

C.

Mormon Crickets.—According to DR. P. N. ANNAND of the Bureau of Entomology, U. S. D. A., an intensive Federal campaign against the Mormon cricket, an insect pest that drove many early western settlers from their homesteads and still causes heavy crop losses, will be conducted this year on infested public lands in the Northern Rocky Mountain region. The Mormon cricket, essentially an insect of the mountains, has been a destructive pest in the United States since 1848. The name "Mormon" was probably adopted because the first recorded appearance of this insect as an agricultural pest was in the Great Salt Lake Basin in Utah, where it overran the first fields planted by the Mormons. Only the coming of myriads of white gulls which devoured the crickets saved the crops from complete destruction. A monument just inside the gates of the temple grounds in Salt Lake City commemorates the timely arrival of these birds.

The Mormon cricket is always present in high, rugged hills of Colorado, Idaho, Wyoming and Montana. Now and then it becomes abundant enough to leave its natural habitat and migrate, in great bands, into the cultivated valleys. These migrating bands may vary in size from that of a city block to a square mile or more. The bands are very dense, often with 100 to 500 crickets to the square foot. Once under way, the band travels in a straight line, stopping at nothing. If it meets a board fence or a house, it tries to climb over the top, rather than going around. A band of Mormon crickets, so the story goes, once entered an open-air pavilion during a dance and advanced across the floor. The crushed bodies of countless insects soon drove the dancers away.

Although Mormon crickets, in general, prefer cereal crops, especially in the doughy stage, they will eat alfalfa, garden crops, small fruits, and, in fact, any plant the farmer grows. The most effective way to prevent damage is to kill the bands of young crickets in the wild lands where they hatch by dusting them with a mixture of powdered calcium arsenite and hydrated lime. If their migration is not stopped in time, the only way to save cultivated crops is to

erect barriers of corrugated tin roofing or galvanized sheet iron. These barriers, although effective in halting the advance and in killing individual insects, are expensive and difficult to handle.

C.

More Dandelions, Less Spinach.—(*Science*, Vol. 80, p. 142.) According to EMMETT BENNETT, in Contribution No. 192 of the Massachusetts Agricultural Experiment station, dandelions in the form of greens are a desirable item of diet. Analyses show the dandelion to have a high protein (15.76 per cent.) and a very low fiber (9.79 per cent.) content, accompanied by a high ash and to contain such minerals as calcium and phosphorus in abundance.

More interesting yet is the discovery that dandelions contain more calcium and phosphorus than does spinach. However, the latter beats out the former in magnesium content. Of all those examined, cabbage leaves had the highest calcium content and lettuce the lowest. Mangold leaves are highest in magnesium. There seems to be no good reason why dandelions could not be cultivated to the extent now enjoyed by spinach.

C.

THE FRANKLIN INSTITUTE

OFFICERS FOR 1934

President	NATHAN HAYWARD
Vice-presidents	HENRY HOWSON C. C. TUTWILER WALTON FORSTALL W. CHATTIN WETHERILL
Secretary	HOWARD MCCLENAHAN
Assistant Secretary and Librarian	ALFRED RIGLING
Treasurer	BENJAMIN FRANKLIN
Assistant Treasurer	MARSHALL S. MORGAN
Controller	WILLIAM F. JACKSON, JR.

Board of Managers

HENRY BUTLER ALLEN
GEORGE H. BENZON, JR.
EDWARD G. BUDD
FRANCIS T. CHAMBERS
G. H. CLAMER
CHARLES H. EWING
SAMUEL S. FELS
WILFRED W. FRY
C. D. GALLOWAY
ARTHUR W. GOODSPEED
CLARENCE A. HALL
GEORGE A. HOADLEY
A. ATWATER KENT
L. H. KINNARD
ROBERT W. LESLEY
HORACE P. LIVERSIDGE
MALCOLM LLOYD, JR.
RICHARD W. LLOYD
CHARLES PENROSE
GEORGE WHARTON PEPPER
JAMES S. ROGERS
GEORGE D. ROSENGARTEN
SAMUEL M. VAUCLAIN

AWARDS BY THE INSTITUTE

The following awards are made by The Franklin Institute:

The Franklin Medal (Gold Medal).—This medal is awarded annually from the Franklin Medal Fund, founded January 1, 1914, by Samuel Insull, Esq., to those workers in physical science or technology, without regard to country, whose efforts, in the opinion of the Institute, acting through its Committee on Science and the Arts, have done most to advance a knowledge of physical science or its applications.

The Elliott Cresson Medal (Gold Medal).—This medal is awarded for discovery or original research, adding to the sum of human knowledge, irrespective of commercial value; leading and practical utilizations of discovery; and invention, methods or products embodying substantial elements of leadership in their respective classes, or unusual skill or perfection in workmanship.

The Howard N. Potts Medal (Gold Medal).—This medal is awarded for distinguished work in science or the arts; important development of previous basic discoveries; inventions or products of superior excellence or utilizing important principles.

The George R. Henderson Medal (Gold Medal).—This medal is to be awarded for meritorious inventions or discoveries in the field of railway engineering.

The John Price Wetherill Medal (Silver Medal).—This medal is awarded for discovery or invention in the physical sciences or for new and important combinations of principles or methods already known.

The Edward Longstreth Medal (Silver Medal).—This medal is awarded for inventions of high order and for particularly meritorious improvements and developments in machines and mechanical processes. In the event of an accumulation of the fund for medals beyond the sum of one hundred dollars, it is competent for the Committee on Science and the Arts to offer from such surplus a money premium for some special work on any mechanical or scientific subject that is considered of sufficient importance.

The Louis E. Levy Medal (Gold Medal).—This medal is awarded to the author of a paper of especial merit, published in the JOURNAL OF THE FRANKLIN INSTITUTE, preference being given to one describing the author's experimental and theoretical researches in a subject of fundamental importance.

The Walton Clark Medal (Gold Medal).—This medal is awarded to the "author of the most notable advance in knowledge or improvement in apparatus, or in method concerning the science or the art of gas manufacture or distribution or utilization in the production of illumination, or of heat, or of power."

The Certificate of Merit.—A Certificate of Merit is awarded to persons adjudged worthy thereof for meritorious inventions, discoveries or improvements in physical processes or devices.

The Boyden Premium.—Uriah A. Boyden, Esq., of Boston, Mass., has deposited with The Franklin Institute the sum of one thousand dollars, to be awarded as premium to "any resident of North America who shall determine by experiment whether all rays of light and other physical rays are or are not transmitted with the same velocity."

For further information relating to these awards apply to the Secretary of the Institute.